610.5

G46
n.S. 19

Library
of the
Academy of Medicine,
Toronto.
8098
Presented by
Dr. N. A. Powell.

Presented to the
ONTARIO MEDICAL
Library Toronto.
By ________

THE

GLASGOW MEDICAL JOURNAL.

EDITED BY

JOSEPH COATS, M.D., and
ALEXANDER NAPIER, M.D.,

FOR THE

Glasgow and West of Scotland Medical Association.

JANUARY TO JUNE 1883.

VOL. XIX.

GLASGOW:
ALEX. MACDOUGALL, 81 BUCHANAN STREET.
LONDON: H. K. LEWIS, 136 GOWER STREET.
1883.

8048.

LIBRARY
DEC 1 9 2002
UNIVERSITY OF TORONTO

THE

GLASGOW MEDICAL JOURNAL.

No. I. JANUARY, 1883.

ORIGINAL ARTICLES.

ON THE PATHOLOGICAL ANATOMY OF TUBERCULOSIS OF THE UTERUS AND FALLOPIAN TUBES.

By JOHN LINDSAY STEVEN, M.B.,
Pathological Chemist to the Western Infirmary, and one of the Assistants to the Professor of Clinical Medicine in Glasgow University.

In what follows I purpose describing the pathological appearances presented by a case of tuberculosis of the uterus and Fallopian tubes, which I had the opportunity of studying during last autumn, while working in the Pathological Institute of Leipzig under the direction of Professor Cohnheim. I am anxious to record the appearances noted, not so much on account of their showing anything very special or unusual, as from the fact of their presenting several points of similarity to, and difference from, those observed in a case which I brought under the notice of the Medico-Chirurgical Society, and which was reported in full in the *Glasgow Medical Journal* for June, 1882. The present case occurred in the course of the ordinary work of the Institute, and I am indebted to the kindness of Professor Weigert for being allowed to undertake the microscopic investigation. I was quite unable to obtain access to the Clinical Records, so that the present account, like the former, deals merely with the pathological anatomy.

The following, obtained from the report of Professor Weigert in the records of the Institute, is a note of the *post-mortem* examination, which was made upon the 15th August, 1882:—

M. D., æt. 24, hawker. The body is emaciated. Both lungs are moderately pigmented and congested. In the apex of the left, however, which is œdematous, a small sharply demarcated caseous area is observed. The heart presents normal appearances. The thyroid gland is enlarged, and on cutting into it numerous small areas of gelatinous degeneration are discovered. In the larynx and trachea nothing special is observed.

At the base of the brain, in the region of the infundibulum, there is an abundant heaping up of miliary nodules, without, however, any injury of the pia mater. The nodules extend into the left fissure of Sylvius, and over the left island of Reil. The right side, however, has very few nodules. The granulations, also, are generally quite discrete. The ventricles are moderately dilated, but nothing else special is noted.

The spleen is firmly adherent to the diaphragm, and partly also to the liver. It weighs 150 grammes, and is quite normal in appearance. The kidneys weigh respectively 135 grammes; in each the capsule is easily removed, the surface is smooth and without granulations, and the pelvis free. In the stomach and duodenum nothing special is noted. The liver weighs 1250 grammes; its lobules are large and brown, brighter at the periphery than at the centre, but not sharply defined from one another.

The small bowels are in part firmly soldered to the margin of the true pelvis, but can be loosened with a little care. The great omentum is here also firmly adherent. After loosening these adhesions, an abundant thick pus flows from Douglas' pouch. The great omentum is specially examined, but no tubercles are found in it. In the cavity of the bowels nothing special is noted until the rectum is reached; and the urinary bladder is perfectly free. In the rectum, about 11·5 cms. above the anus, an opening about the size of a mark is discovered, in the mucous membrane surrounding which abundant white nodules are situated. The opening leads into Douglas' pouch, which is filled with pus, and whose walls are beset with very abundant caseous nodules, varying in size from a millet seed to that of a pea.

The vagina is free, but from the mucous membrane surrounding the os uteri a little somewhat broken nodule projects. The cervix is still partly covered with mucous membrane, but in some places is beset with fatty and caseous masses. The cavity of the uterus itself is filled with pus, the mucous membrane is fatty and caseous, and on section of the wall here and there, distinct miliary nodules are seen. The left Fallopian tube is greatly thickened and elongated.

On section thick caseous material flows out, and the wall is studded with caseous fatty masses. The right tube is less severely involved filled with thinner pus, and its walls are not so caseous. The glands in the neighbourhood are much swollen, and in them scattered miliary caseous nodules are discovered.

Diagnosis.—Tuberculosis of the uterus and of the tubes—circumscribed peritonitis—tuberculosis in Douglas' pouch—ulcer in the rectum—tuberculosis of the retro-peritoneal glands—tubercular meningitis.

Such is the account of the *post-mortem* examination and the naked eye appearances. The parts subjected to a more detailed microscopic investigation were the uterine wall, the Fallopian tube, and the sub-peritoneal glands, and in what follows is given an account of their minute structure:—

The sections examined were cut for the most part by a microtome known as the "Cohnheim machine," and made by the mechanic attached to the Institute. It possesses, as it appears to me, two great advantages over the microtomes ordinarily in use in this country—viz., hardened tissues are not frozen before they are cut, and thus one of the greatest obstacles to thorough staining of the section is removed; and, secondly, the instrument is provided with a most accurate apparatus for graduating the thickness of the sections cut. The staining agents employed were Bismarck brown, Gentian violet, eosin, and picrocarmine.

In the uterus the morbid changes were found to be almost entirely limited to the region of the mucous membrane. Here and there areas of normal mucous membrane, consisting of masses of small, somewhat elliptical-shaped epithelial cells, could be made out, and sometimes remains of the uterine glands could be seen. For the most part, however, the mucous membrane was found to be replaced by, or converted into caseous masses, which often at their marginal parts were surrounded by a dense fringe of cells partly round and partly spindle-shaped. Where caseation was not so advanced, areas of almost simple granulation tissue were noted, and in a few situations distinct tubercles were found. The morbid conditions did not extend to any great extent, if at all, into the muscular part of the uterine wall, which was mostly normal. No changes were noted in the serous coat of the organ.

In the Fallopian tubes, almost precisely the same conditions as were noted in my previous case were found. The whole process was almost entirely confined to the mucous membrane, the serous and muscular coats being free. In place of the mucous membrane, large areas of caseous material, which were often separated from the muscular coat by a layer of granulation tissue, were observed. Unlike the last case, however, no tubercules were observed on the serous coat. As has been noted in the *post-mortem* report the tubes were considerably enlarged, but they were not nearly so much so as in the former case.

The much enlarged pelvic lymphatic glands were next examined. The true gland tissue was found to be very largely replaced by caseous masses, and here, as in the mucous membrane of the uterus, these masses were often surrounded by a very dense fringe of variously shaped cells, looking indeed as if it was in them that the advancing caseous changes were taking place. In some situations the normal gland tissue could be made out, and also arrangements of cells very suggestive of the occurrence of discrete tubercles.

The next step in the investigation was to institute a very careful search for the tubercle bacillus. The method adopted for this purpose was that of Dr. Huber, one of the assistants to Professor Cohnheim, which consists in the use of a solution of gentian violet in aniline oil water of the strength of 1 to 30. The section is immersed in this for twelve or more hours, then treated with nitric acid (which extracts the colour from everything except the bacilli, leaving them of a deep violet colour), and mounted in the usual fashion. For a long time the search was like to prove fruitless, but at length, after I had examined a large number of sections treated in the above manner, I discovered in some of the sections a few very minute rod-shaped bodies of a violet colour, which I had no doubt were the bacilli. I submitted one of these sections to Professor Weigert, who examined it with one of Zeiss' $\frac{1}{12}$ oil immersion lenses. His opinion was that, although very scarce, the bacilli were present. Thus, both by its anatomical characters, and the presence of bacilli, the case has been proved to be one of true tuberculosis of the uterus, Fallopian tubes, and surrounding structures.

I have now briefly to compare the case just described with that which I formerly recorded. First of all then, as regards the nature and character of the morbid changes present in the tissues, the cases may be said to be almost identical. In both the appearances were typically those of tubercular disease, and the tissue mainly involved in the morbid change was the mucous membrane of the affected organs, although there were also tubercular nodules on the serous coverings. This, however, is nearly all that can be said for the similarity of the two cases, as in most other points they differ considerably. In the former case the disease was confined very largely, if not entirely, to the mucous membrane of the Fallopian tubes, excepting always the miliary nodules present on their serous coats and on the peritoneal covering of the uterus, the latter organ, with this exception, being free. In the present case the disease had invaded the mucous membrane of the uterus as well as the tubes, the uterus being more involved than the tubes, and had largely spread to neighbouring structures—Douglas' pouch, the rectum, lymphatic glands, &c. Again, with regard to the general nature of the two cases, there are very striking differences. In the first case there was very grave tubercular disease, involving very extensive condensation of both lungs, and in one excavation, with, in addition, severe general tubercular peritonitis and enlargement of the mesenteric glands. In that now under review, the only situations excepting the pelvis, where evidences of tubercular disease were detected (and the whole body was carefully examined), were the apex of the left lung, where a small and well defined caseous nodule was found, and the base of the brain, the disease of the latter, however, having been so slight as not to have caused "injury to the pia mater." Lastly, it is to be noted that in the former case the Fallopian tubes were much more extensively diseased towards their free extremities than nearer the uterus. In the present this was not so.

The comparison, then, of the two cases proves, I think, the following points—viz. (1) that, as I have shown in my previous paper, a tuberculosis of the Fallopian tubes, and of them alone, may arise from the action of infective material sucked into them from the peritoneal cavity, and this, which may be looked upon as a secondary condition, is most likely to occur where grave tubercular disease exists elsewhere; and (2) that a tuberculosis of the internal female generative organs may occur primarily in them, or at least, throughout the entire case, may remain most severe in them, and this is likely to be the condition when no, or as in our case only very slight, evidences of tubercular disease exist elsewhere. Only the latter form of the disease is likely to be of importance in practical medicine, as in the former the tubercular lesions elsewhere than in the generative organs occupy the whole attention of the physician. With regard to the latter, then, I would conclude this paper with the practical remark that possibly the diagnosis of such cases might be greatly aided by the examination of the discharge for the tubercular bacillus.

We know that the bacilli, when proper reagents are used, and moderate care exercised, are easily demonstrated in the sputa of patients suffering from tubercular lung affections, and, although I have not yet had an opportunity of carrying out such an examination, I think they could be demonstrated in the discharges from a tubercular uterus.

ON SOME OF THE ADVANCES WHICH HAVE BEEN MADE IN SURGERY DURING THE LAST DECADE.

By JAMES WHITSON, M.D., F.F.P.S.G.,
Surgeon to the Dispensary of Anderson's College; late Extra Dispensary Surgeon, Glasgow Royal Infirmary.

(Read before the Glasgow Royal Infirmary Medical Society, 18th November, 1882.)

Mr. President and Gentlemen,—During the last few years surgery has advanced with rapid strides, the practices of our forefathers having in many instances been laid aside, while new and improved methods of conducting operations and of subsequent treatment have been adopted in their room.

The introduction of antiseptics is undoubtedly one of the most potent factors in effecting these changes, and it is but just to state that Lemaire was the first to use carbolic acid in the treatment of wounds, and to realise the truth of the germ theory, in regard to which he says, "The wound which suppurates, as I hope to demonstrate presently, is a secreting surface, the products of which become altered under the influence of air, and give birth to a series of phenomena due to one and the same cause—fermentation." Lemaire, however, did not systematise his method of procedure, and, as Cheyne* points out, he had no definite aim, no clear purpose in his work. To Mr. Lister we owe the gradual development and improvement in the antiseptic treatment, and those of you who are now familiar with the almost perfect exemplification of this system in the wards, can have little idea of the difficulties which attended its early stages, or of the crude methods which were then devised for its being carried out. Mr. Lister used to tell his class that his attention was first directed to the matter by noticing the easy manner in which simple fractures healed as compared with compound. Previously to the introduction of antiseptics the latter ended generally in amputation, it having been found by experience that the removal of the affected limb gave the best chance of ultimate recovery. The first experiments bearing on the subject were made by this eminent surgeon on compound fractures.* Carbolic oil of one to twenty strength was poured into the wound, and to prevent its evaporation an external dressing of carbolised putty and tin was placed over it. It was not till the spring of 1871 that gauze was introduced. Being then a resident assistant in this house, I wrote Mr. Lister asking for a sample, and in a short time he kindly complied with my request, adding that he had delayed sending it until he could prove it thoroughly trustworthy. Sprays were at this time unknown, and the only method of protecting a wound from the influence of the air was by keeping a gentle stream of carbolic acid and water (one to twenty) playing over its surface by means of a syringe. About ten years ago foot sprays were introduced, but were found laborious as well as troublesome to work, the slightest neglect of the person in charge being liable to do mischief.

---

* *Antiseptic Surgery*, by W. Watson Cheyne, M.B. Page 356.

Even with these rough methods results were got so highly satisfactory, as to warrant the belief that the system only needed further investigation to show that it was destined to play a most important part in the future of surgery; and all the world now knows how successfully Mr. Lister has accomplished this, and how gradually, step by step, he has improved the system, until at the present time results well nigh perfect are obtained.

Well, gentlemen, when I first became a student of this hospital, sixteen years ago, the methods of treatment then in vogue were totally different from what you are now accustomed to see. In amputating a limb bloodless surgery was unknown, and the compression of the main artery was entrusted to the care of an assistant. In this way a good deal of hæmorrhage took place, for even granting that the chief vessel was firmly held the smaller anastomosing branches would remain patent until ligatured. The application of the tourniquet, which works by means of a screw and strap, was not much better, for though it arrested the flow so long as it was retained in position, a considerable quantity of blood was imprisoned below the seat of amputation, and thus practically lost. The tissues at the same time were gorged with it, and oozing, as a natural consequence, would take place for some hours after division of the muscles. In these cases, instead of a serous or bloody discharge not usually extending beyond the first week, there was always heavy and long continued suppuration, and as pus, from its viscid consistency, does not obtain a speedy exit from stumps, the consequence was that healing took place somewhat slowly, at the same time weakening the patient and retarding recovery. In addition to all this there was the danger of absorption of poisonous material into the system. Epidemics of pyæmia and hospital gangrene* were of frequent occurrence, and when these dread diseases entered a ward the ranks were decimated in an alarming manner, while fumigation and other hygienic remedies did not prevent their return. Even erysipelas made its appearance much more frequently than it does now, and as wounds were generally left open it quickly spread to others in its neighbourhood.

---

* "On a New Method of Treating Compound Fractures, Abscess, &c." By Joseph Lister, F.R.S., *Lancet*, 16th March, 1867. Page 327.

At this time the drainage of wounds had received but scant attention, and in amputations the operator aimed at cutting his flaps in such a way that there was a natural or dependent outlet for the discharge, and by the method of Teale, with its various modifications, this to a certain extent is accomplished.

Silk ligatures were used in securing blood-vessels. The ends were left long and brought out at either angle of the stump, so that when the process of ulceration was completed they could be removed piecemeal. In a way they acted as drains, but it is not difficult to see that a porous body such as silk, left for some time in a suppurating wound, must become thoroughly soaked with decomposing material, and as a natural consequence prove at the very least a source of irritation. The practice of passing a piece of rag between the flaps was equally bad, and instead of serving the purpose of a drain must rather have tended to retard the object for which it was introduced.

Lint was the dressing used for stumps,† and in a short time after being applied it became soaked with pus, and consequently the wound was kept almost continually bathed in a fœtid discharge. The smell of the wards in these days was often overpowering, and if once felt was not likely to be forgotten.

---

* "On the Effects of the Antiseptic System of Treatment upon the Salubrity of a Surgical Hospital." By Joseph Lister. *Lancet*, 1st January, 1870. Page 5.

† "On the Effects of the Antiseptic System of Treatment upon the Salubrity of a Surgical Hospital." By Joseph Lister. *Lancet*, 8th January, 1870. Page 40.

Let us compare all this with the practice adopted at the present day under the antiseptic regime. Bloodless operations are now generally carried out in order to husband the patient's strength, and give him every possible chance of recovery. In amputations the best way of accomplishing this is to roll an elastic bandage round the affected limb, beginning at the distal extremity and gradually extending as far up as is necessary, where it is held by an assistant until it can be uncoiled from its lower end and carried upwards to a sufficient extent for operating, when it is finally wound round its highest folds and fixed by a safety pin in the usual manner. In this way the vessels are completely emptied, and hæmorrhage during the operation is efficiently controlled.

This method is vastly superior to the one adopted by some surgeons of stretching an elastic rod or band round the limb, and keeping it to the requisite amount of tension by means of the catch of Foulis, for not only does it fail to empty the parts of blood, but its compressing power is exercised over much too limited an area, and in one instance I know of, where the band was retained for several hours above the elbow during the transit of a patient from the country, so much injury was done to the tissues in the neighbourhood that amputation at the shoulder joint became necessary soon afterwards.

Much greater care is spent in the arrest of bleeding than formerly, and the more thoroughly this is done the less discharge there will be, with a correspondingly diminished risk of tension, and, consequently, the more speedily will the wound heal. Nowhere is the old maxim, "The more haste, the worse speed," better verified than here, and a little time is well spent in arresting bleedings from even small arterial twigs.

The catgut ligature is an immense improvement on the old fashioned silk one. It is bland and innocuous in its character, and capable of absorption by the tissues without injury to the patient. In the deligation of arterial trunks in their continuity in the operation for the relief of aneurism, it has proved itself reliable, and may be cut short and left in the wound. As a suture gut is preferable to all others. It permits of a nice adaptation of the parts, is not liable to catch on the dressings when these are being removed, and can be left to take care of itself with no fear as to ultimate results. In the radical cure of hernia the pillars of the ring can be brought together with strong threads of catgut, and it is much superior to the very thick wire used by some surgeons for this purpose. Having operated recently on a case of this kind in Ward 29, I am in a position to say that it answered my expectations thoroughly, and the difficulty in the way of removal of thick wire constitutes one of the most serious drawbacks to its use; while the practice recommended by Mitchell Banks* of Liverpool, of leaving it buried in the tissues must occasionally give rise to after discomfort, for it is impossible that a heavy metallic suture, retained in such a position, can at all times accommodate itself completely to the parts in its neighbourhood. For the apposition of periosteum gut is indispensable, while hæmorrhage from bone can be completely arrested by plugging the cavity with threads of this substance, and Lister's sinus forceps will be found the most efficient agents for accomplishing it. For all these purposes gut hardened in chromic acid should be used, and if prepared by the process recommended by Macewen† it is thoroughly reliable, and will be found capable of resisting the action of the tissues for a considerable time. I have on more than one occasion found to my cost, that the material sold in shops, under the name of "chromic catgut," was in truth nothing of the sort, and gave way in the course of two or three days.

The drainage of wounds is now recognised as one of the most important elements in their successful treatment, for if the discharge does not get a free exit tension must follow, which in its turn gives rise to inflammation, and if these remain unrelieved for a sufficient length of time suppuration is the inevitable result.

The india-rubber drainage tubes of Chassaignac were the first agents employed for this purpose. The material of which they are composed is non-porous, and does not absorb the fluids which come in contact with it, and unless compressed there is always a patent channel through which fluid material of any kind can pass. Small holes are pierced in the tubing at short intervals in order to increase its action, and if necessary it can be removed at each dressing, thoroughly cleaned, and then replaced.

More than two years ago decalcified drainage tubes,‡ made from the tibiæ and femora of the common fowl, were introduced by Macewen. These fulfil their purpose very well, and taking advantage of the principle of capillarity are threaded with horse hair, which, while maintaining their calibre, forms at the same time an excellent conductor of a serous or sanguineous discharge. The hair is removed at the end of forty-eight hours, but the tube itself needs no further care, and is generally absorbed either whole or in part.

---

* "On the Radical Cure of Hernia." By W. Mitchell Banks, M.D., F.R.C.S. *British Medical Journal*, 18th November, 1882. Page 985.

† For further information on this point, see *British Medical Journal*, 29th January, 1881.

‡ *British Medical Journal*, 5th February, 1881.

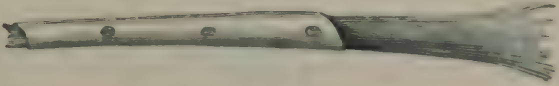


FIG. 1.

The above gives an excellent representation of a Decalcified Drainage Tube, threaded with horse hair.

A further improvement, which has been carried out in amputations by the same surgeon, is the piercing, by means of a pair of dressing forceps, of the posterior flap in one or more places, and the securing in these of the tubes by means of stitches of catgut. No method can be adopted which more effectually fulfils its purpose than this, and a wound is likely to be most completely drained when several exits are afforded for the escape of discharge. The same principle leads the agriculturist to put in a number of small drains in preference to one large one, when conducting operations for the removal of superfluous moisture from his fields.

In bringing the edges of a wound together it is not only requisite to do so accurately, but to avoid tension on the parts. The introduction of button sutures has greatly tended to relieve this, and we have three different kinds to choose from, those of Ogilvie Will, of Lister, and of Macewen. The first two are for wire alone. The one devised by the last named surgeon is to my mind much the simplest and best, and while easily and cheaply made, is equally well adapted for either wire or catgut.

In many situations gut is the most convenient thread to use, for the ends of one side can be left long without inconveniencing the patient, and when secured by a slip-knot, *bb*, Fig. 3, a greater or lesser amount of pressure, according as circumstances demand, can be applied at any time without disturbing the parts in an injurious degree. Those who have had to deal with wire in this relation know, that though fulfilling its function efficiently, it is not easily untied, and the ends, whether left long or cut short, do not adapt themselves to the parts in their neighbourhood with the pliancy of gut. The button suture should be made use of first, and as the edges of the wound are thus brought into contact, the subsequent passing of the stitches, of which there are two kinds, can not only be accomplished easily but efficiently.

For example, in the removal of a large adenoid sarcoma of the mamma, which many of you saw me undertake recently, there was a considerable gap to fill up, and the method I adopted of

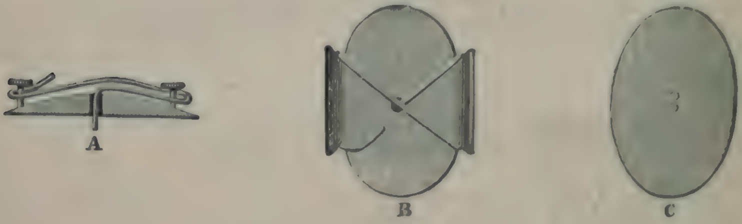


Fig. 2.

A. The Button of Ogilvie Will.—The wire is passed through the aperture in the centre, hooked round the pillar of one end and carried onwards to that of the other.

B. The Button of Lister.—The wire is passed through the hole in the middle, twisted round one of the everted sides and then round the other.

C. The Button of Macewen.—A double thread of wire or gut is passed through the two small openings in the centre. If the former is used, the ends are secured by twisting the one round the other in the usual way with this material, and with the latter a reef-knot (*aa*) on the one side and a slip-knot (*bb*) on the other complete the arrangement, as shown in the following figure. With this button the amount of pressure which it is thought desirable to apply can be regulated with the greatest evenness and nicety, and there is no tilting of the sides.

Will's Buttons are made of silver, and consequently are expensive. Lister's are of lead. Macewen's are made of block tin. They possess more pliancy and adaptability to the shape of the parts in their neighbourhood than either of the others. To prevent friction protective plaster should be placed between them and the skin, and in this case there is no liability to the throwing down of a black deposit as usually happens with lead.

bringing the raw edges together was as follows:—Two button sutures of thick gut were passed first of all, *aa*, *bb*, Fig. 3, so as to approximate the bases of the flaps. Several deep stitches of the same material placed at regular intervals followed, which took a good grasp of the tissues, and are termed the stitches of relaxation; and finally, between these were inserted a number of threads of fine gut, termed the stitches of coaptation. The strain on the parts was in this way relieved, the edges were closely and evenly approximated, and union by first intention took place along the whole line of incision.

In the figure below an excellent illustration is given of the best method of bringing together the raw edges in the amputation of a mamma, and as performed in the way just narrated. The letters *aa* represent the button sutures (catgut), tied by means of a reef-knot, and the letters *bb* show the same threads secured at their other end with a slip-knot. At *c* are seen the decalcified drainage tubes threaded with horse-hair, and retained in their places by means of catgut stitches. Four stitches of thick catgut (stitches of relaxation) are placed at regular intervals along the line of incision, and between these are inserted a number of threads of fine gut (stitches of coaptation).

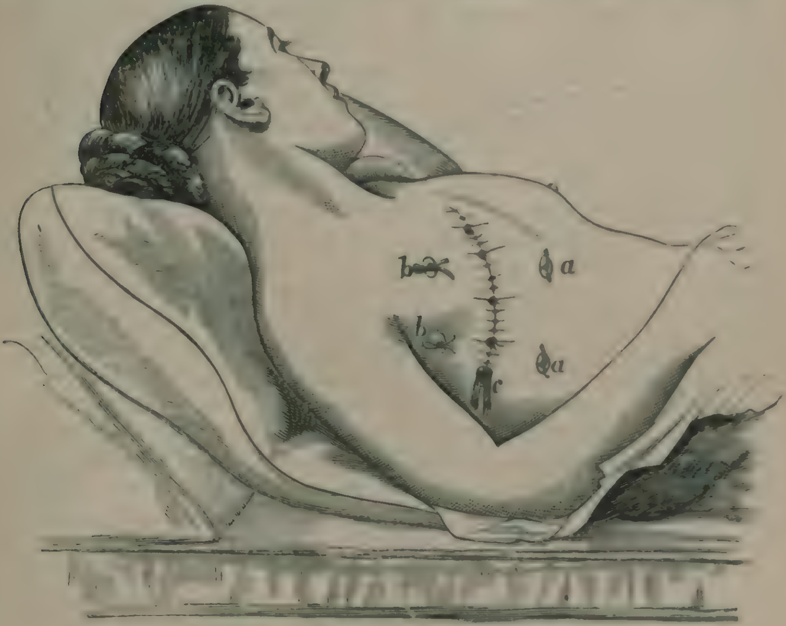


Fig. 3.

The scar which followed this wound was thus reduced to a minimum, and in a short time, unless closely looked into, would probably pass unobserved.

The gauze dressing, with which you are all familiar, is a great improvement on that of lint. It thoroughly protects the part it covers from all external influence, it does not require so frequent changing, and it has much more absorbent power—

three very important points to be borne in mind as regards the wellbeing and speedy recovery of the patient.

The operation of osteotomy, devised and most successfully carried out by my friend, Dr. Macewen, for the relief of certain deformities of the lower limbs, is a great advance made in surgery, and to have attempted such a thing a few years ago would have been considered nothing short of madness. The statistics* of this eminent surgeon, however, prove that in his hands it is a safe procedure, resulting in benefit to the patient, both as regards appearance and powers of locomotion. The operation mainly consists in the division of bone, and in order to accomplish this either sawing or cutting must be resorted to. The latter is the more preferable, as well as the easier, method of effecting this, as in sawing there is not only laceration of the soft tissues, but a considerable amount of bony debris must be left in the wound, which is apt to lead to suppuration. On the other hand, when using the chisel in the proper way, the bone is divided without much difficulty, and beyond the first incision which, if properly performed, ought to be a clean cut, there can be little or no injury done to the soft parts.

Excepting cases of osseous anchylosis, osteotomies are performed mainly for the relief of three kinds of deformities—knock knees, bow legs, and anterior tibial curves. In the first class the femur is divided on its internal aspect, at a point opposite, and corresponding to, one inch above the external condyle. In the second the tibia, and if need be the fibula, are osteotomised in one or more places, but in the case of children it is not always necessary to accomplish the latter proceeding, as after section of the tibia the fibula will frequently yield to pressure, a green stick fracture being produced. Where there are prominent anterior tibial curves, the best way is to remove a wedge of bone, after which straightening of the limb will be found comparatively easy. The tibia is much harder and denser than the femur, and requires a good deal of force to sever it. When rickets is present this characteristic is especially well marked. In all osteotomies it is highly desirable at the first to get the chisel well into the bone with a few vigorous strokes of the mallet. In this way not only is division rendered easier, but there is much less chance of the instrument getting out of the groove, and in consequence excoriating the periosteum or otherwise injuring contiguous structures.

---

* *Osteotomy, with an inquiry into the Ætiology and Pathology of Knock Knee, Bow Leg, and other Osseous Deformities of the Lower Limbs.* By William Macewen, M.D. Page 161, chapter xxi.

In the treatment of the various kinds of talipes, the cutting of the offending tendons, coupled with the after wearing of steel boots or other mechanical appliances, is the only curative measure followed by many surgeons. The division of the tendons no doubt aids in the rectifying of the mal-position, but unless the foot can be subsequently kept in its proper place tenotomy is of little use, and the old habit speedily regains the ascendency. Theoretically, the wearing of steel boots may appear easy and efficacious, but practically they often give rise to much discomfort, and as the pressure which they exercise is confined to a somewhat limited area, abrasion, and if neglected, even sloughing may take place in consequence. The application of splints is frequently unsatisfactory, as the constant movements of the child soon render the whole apparatus loose, and consequently ineffectual for accomplishing the object we have in view.

The introduction of paraffin has, however, been turned to good account here, and many cases of club foot can be cured without division of the tendons by the wearing of a boot of this material for a sufficient length of time. I have frequently practised the method recommended by Macewen, and with excellent results. It may be described as follows:—The patient having been put under the influence of chloroform, a gauze bandage is wound round the limb, after which a sheet of the ordinary coarse wadding of the requisite size, previously soaked in liquid paraffin, is moulded over it. The foot is now forced into as favourable a position as possible, and kept so until the process of solidifying has taken place, which may be hastened by the action of cold water. The boot can be worn for six weeks, when it may be taken off, and at the end of this period, if the foot should still tend to revert to something of its old habit, the paraffin can be re-applied.

It may be fairly claimed for paraffin, in the treatment of talipes, that while thoroughly effectual, it is easy of manipulation, and almost always well borne by the patient. After being applied to the limb it is not liable to subsequent displacement, so that the foot, when once securely fixed, cannot resile to its original position. Movement on the part of the child does not matter, and as the pressure which the boot exercises is equally diffused, it seldom happens that any injury is done to the skin. I have invariably found that age is a most important factor as regards the after result, and the younger the child the more hopeful is the prognosis. When patients are allowed to walk before being operated on, a partial cure is in many instances only to be looked for, and much time and pains are demanded in securing even this.

In obstinate cases of talipes varus, the removal of a wedge from the astragalus and os calcis is frequently followed by improvement, and it is not difficult to see that the gap which is so produced must permit of the foot being everted into a more favourable position than it could otherwise occupy.

A totally different method of dealing with abscesses is now carried out as compared with the practice of even recent years.

Formerly, after incising the cavity and pressing out the contents, the case, with the exception of frequent syringing, was almost entirely left to nature, while recovery in most instances was tedious. At the present time, and with antiseptic precautions, a much more vigorous line of treatment is adopted, and we have no hesitation in removing the pyogenic membrane *en masse*, interference with which older surgeons looked on with disfavour. In order to accomplish this we make use of Volkmann's spoon, and if the whole of the inflammatory products contained in the cavity are scraped out, two healthy surfaces are left apposed to each other, and which speedily becoming covered with granulations soon coalesce. During the process of healing the less the parts are disturbed the better, and syringing the cavity irritates the tissues composing its walls, and leads to increased discharge—while the progress of cicatrization is interfered with, and as a natural consequence recovery, instead of being accelerated, is considerably retarded.

The spoons of Volkmann are extremely useful instruments, and will be found invaluable for the removal of diseased granulation tissue, and similar purposes. They are made of different lengths, and the head of the spoon can be procured in either a round or oval form.

The transplantation of bone* is now an accomplished fact, the greater part of a humerus having been replaced by Dr. Macewen during the last year or two. Many of you may remember of having seen the case alluded to in Ward 22, in which the most of the shaft of the bone had become necrosed, and it was thought that amputation was the only course left. Before proceeding to that extremity, however, Dr. Macewen thought it would be as well to try if living bone could not possibly be transplanted from one subject to another, and taking advantage of the wedges removed in tibial curves he made the attempt, and, as all the world now knows, successfully. I may mention that when I had charge of Dr. Macewen's wards, during August, 1881, this patient, William Connell, was an inmate of them. I had thus daily opportunities of seeing the various movements he could perform by means of his new humerus, and at the present time there is a most serviceable arm.

---

* *Proceedings of Royal Society*, No. 213. 1881.

In excising the elbow joint a marked improvement has been suggested by Ollier, the eminent surgeon of Lyons. This consists in securing the periosteum by means of its reflection backwards before removing the diseased bone, and its subsequent apposition with stitches of catgut. The method is valuable for the retention of muscular connections.

In resections, abscess cavities are frequently found to exist in the neighbourhood of the articulation. These should be thoroughly cleared out at the same time by means of Volkmann's spoon. In this way sound material alone is left, while healing is not only accelerated, but a better ultimate result is obtained than when the periosteum was taken away, and inflammatory products allowed to be discharged from the wound by a slow natural process.

Well, gentlemen, I have endeavoured to put into as concise a form as possible a few of the more important improvements which have recently taken place in surgery; and though the paper has cost me a good deal of time, and a good deal of labour, yet I shall feel amply rewarded if any of the numerous friends whom I now see before me have in any way benefited by the reading of it. Let us hope that when the end of the next decade comes round, all of you who are here present may be spared to see still further advances made, that the means of coping with disease, medically as well as surgically, may be more effectual, and that scientific research may bring to light many remedies of whose existence we are at present ignorant, and on whose certainty of action we shall always be able to place the most implicit reliance.

Gentlemen, the future is to all of us a sealed book, whose pages no analyst can examine and no magician lay bare; but let me beg of you, as you go through life, to use all your energies for the promotion of high and noble ends, and to advance the honour and dignity of the profession by every means in your power.

A CASE OF IMPERMEABLE STRICTURE OF THE URETHRA, TREATED BY PERINEAL SECTION ACCORDING TO WHEELHOUSE'S PLAN.

By ANDREW MARSHALL, M.D., PRESTON.

ON 3rd November, 1881, I was requested to see the following case in consultation with two medical friends.

The patient, aged 27, had suffered from gonorrhœa several years before, and ever since had experienced more or less difficulty in micturition. In August, 1881, three months before I saw him, he unfortunately contracted a second attack which he foolishly neglected, notwithstanding repeated warnings from his medical attendant. Cystitis supervened for which he underwent some treatment. Subsequently a hard swelling appeared in the perineum. Œdema and inflammation of the scrotum and penis followed, together with a red blush and some tumefaction above the groins. A considerable portion of the integument of the penis sloughed and separated, and the patient's urine, more or less mixed with pus, was afterwards regularly passed through an opening thus formed on the dorsum of that organ.

Careful and prolonged attempts had been made at various times to pass an instrument through the urethra into the bladder, but without success. It is unnecessary to add that the patient's health had suffered severely.

At the time of my visit the nature of the case was manifest. The man was very ill, and anxious for relief. There was no gonorrhœal discharge from the urethra. A large granulating sore existed on the penis. A long probe introduced through the opening on the dorsum easily passed upwards and outwards towards the left groin into a cavity situated under the integument there, and another passed without much difficulty downwards to the perineum. There was no external opening in that situation. On the patient attempting to empty his bladder, the cavity in the groin immediately became distended, and then a mixture of urine and pus issued from the dorsal opening in a stream corresponding very closely in size to the natural one. Here, then, in addition to a secondary cystitis, there was an organic stricture which had been found to be practically impermeable, behind which a urethral abscess had formed, and, communicating with the urethral canal had been followed by the usual results: extravasation of urine with consequent sloughing of tissue;

and the urine leaving the proper channel through an opening behind the stricture, passed by an abnormal and circuitous route up to the groin, and then under the skin to the penis, from which it escaped as mentioned above.

After consultation, we determined, without delay, to attack the stricture which, of course, was the cause of all the existing mischief. I tried to penetrate it by some soft elastic instruments, as well as by Lister's metallic bougies, but failed. It was therefore agreed that I should do perineal section on the following day. Accordingly, at the time appointed, chloroform was administered and the operation performed according to Wheelhouse's directions. His staff having been passed, the preliminary dissection was easily done, and the urethra speedily opened in front of the stricture which existed in the usual situation. The sides of the urethra were then held apart by catch forceps. But very profuse and general oozing of blood occurred from the cut surfaces, especially from the deep portions of the wound, which, by obscuring the parts, rendered the task of finding the minute opening into the stricture, and introducing the probe pointed and grooved director, a very tedious and difficult matter. At last, however, this was accomplished, and the hard cicatricial tissue divided. Lund's widely grooved director was then introduced through the wound backwards into the urethra, and a large catheter passed through the meatus guided by it into the bladder. This was kept in for a few days, and afterwards passed from time to time. The proper way out for the urine having been made patent, the abnormal passage and cavity healed up, and a good recovery ensued in due time.

Mr. Wheelhouse's plan of operating adopted in this case is, I think, a decided improvement on the older plans. Mr. Syme's operation was not practicable, for no instrument could be got through. His well known doctrine that no stricture is truly impermeable is doubtless perfectly correct in theory, and will generally be found so in practice by a careful and painstaking surgeon; but occasionally a stricture will be met with so tight, or so tortuous, or so complicated, that no instrument passed down through the urethra can be made to penetrate it. For such a case Wheelhouse's operation is probably the best, although it is not by any means an easy one, for in few cases really requiring perineal section will the orifice of the stricture be easily found or an instrument easily got in. Mr. Wheelhouse recommends Teale's grooved gorget as a guide for the catheter into the bladder, but Lund's director seems to me a more serviceable instrument.

A LECTURE INTRODUCTORY TO A COURSE ON AURAL PATHOLOGY.

(Delivered in the Glasgow Royal Infirmary, 1877-78.)

By JAMES PATTERSON CASSELLS, M.D., M.R.C.S. Eng.,
Formerly Aural Surgeon to the Royal Infirmary, and Lecturer in the Royal Infirmary Medical School.

GENTLEMEN,—As the necessity for a special study of the pathological phenomena of the diseases of the ear, in regard to a comprehension of their causation, diagnosis, prognosis, and treatment, has already been considered by me in my introductory address, it is not deemed necessary to dwell upon that point again; but I cannot resist this opportunity of reverting to the subject, just to say that such a knowledge as I desiderate, and which I hope to be able also to impart to you, is the basis of all sound diagnosis, prognosis, and even of treatment, in regard, not only to the diseases of the ear, but in respect also to the diseases of the body generally.

In illustration of this position, and also with the object of showing to you the value of a special knowledge of the diseases of the ear to general practitioners, let me relate to you, in a brief manner, the history of a case that occurred within my own experience some years ago.

A lady, of about 65 years of age, of a nervous temperament, and highly intellectual and refined in mind and manners, became suddenly deaf. Her medical attendants diagnosed, but on uncertain grounds, that the deafness was due to a blocking up of the meatuses by collections of cerumen, and in accordance with this opinion as to the nature of the case, they proceeded to remove the obstruction from these canals by syringing them out with tepid water. But not succeeding in their intention by this operation, which was repeated several times a day, for many succeeding days, without further change in the state of their patient, the medical gentlemen began, as was natural under the circumstances, to doubt the correctness of their diagnosis. Accordingly, it was now determined by them to treat the lady for some supposed infirmity of the nerve of hearing, and, with that object in view, she was ordered to be confined to her bedroom, and to be prohibited from receiving the visits of any one, except those of her household who were in waiting upon her. For several weeks this plan was followed out most rigidly without success. The lady, who was fond of society, now became more and more nervous and irritable, in consequence partly of the secluded life that she was forced to lead, and in part also of the worrying irritation of the subjective tinnitus from which she constantly suffered, till at length grave fears began to be entertained that her mind "might give way" under the load of her miseries. At this juncture in the case, it occurred to the lady's husband to suggest a consultation.

This was accordingly had, and the result of it was, that in less than twenty-four hours the lady was seated at the head of her dinner-table, hearing quite well.

Now, you naturally want to know what had been done to the lady to effect so great and so marvellous a change in her condition. At the consultation that was held, it was found that the first diagnosis formed was a correct one, and that the ordinary attendants had only failed to remove the accumulated matters from the meatuses through a lack of the knowledge of some facts which they would undoubtedly have known and acted upon had they possessed a knowledge of the pathology of the organ with which they were dealing. The fact was that they did not profit even by the history of the onset of the deafness, as they abandoned their first formed diagnosis in favour of one that was untenable in the face of this history.

The deafness was *sudden* in its onset, a fact that was consistent with the occurrence of deafness from accumulations of cerumen in the meatuses, and quite untenable on the assumption that the deafness in this case was due to some form of disease of the labyrinth. Doubtless there are cases of gradually increasing deafness often met with that are due entirely to accumulations of cerumen, and which are cured at once by its removal, and also there are cases of congestion of the labyrinth, in which the loss of hearing is at once sudden and complete; but this lady's case had nothing in common with either of these.

Then, apart from the history of the case, they were not aware of the fact that in advanced life the orifice of the meatus, which is composed of cartilage alone, collapses, and that this is oftenest met with in *females*, in whom, also, it is very pronounced and complete.

Nor were they aware that this valve-like condition of the collapsed tissues of the orifice of this canal effectually hinders the entrance of water into it, no matter although great force is used in the attempt to do so, or that the obstacle, which effectually baffled their efforts to remove the cerumen and other matters from these canals, could have been at once set aside, in great part, although temporarily, by simply making traction on the free margin of the auricle, in a direction upwards, outwards, and backwards.

If the relation of this case has not taught you one or two facts in regard to the pathology of the diseases of the ear, as well as showed you the value of such special knowledge, no matter what may be the future sphere of your labours, I have related it in vain. I hope it is not so, and that it may have taught you, at least, that in executing even so simple an operation as that of *syringing out the external auditory canal*, there may be required special knowledge on the part of the operator, ere he can successfully accomplish it.

In speaking upon the importance of a study of aural pathology, it would be an omission on my part did I not tell you that all that I have said on this point is borne out by a study of the history of the progress of aural surgery in this and other countries, during the last three-quarters of a century.

From the beginning of the present century till now, the student of its history must notice the fact, that the remarkable advances which have been made in regard to greater precision in diagnosis, and to better methods of treatment, have been preceded in every instance by equally remarkable and lasting additions to our pathological knowledge of the diseases of the ear. By and bye, I shall show you also, that even *general* pathology has contributed of its store to the elucidation of some obscure and hitherto unsolved questions in respect to the etiology of ear diseases.

For all, or the greater part of this special pathological knowledge of ear disease, we are indebted mainly to such men as Saunders, who was the father of English aural surgery. In 1806 he published a work on the *Causes of the Diseases of the Ear*, so excellent in its matter as to justify the reproduction of the book, without any substantial change, even at the present day, so accurate are the pathological observations that are made in it.

To Toynbee, also, who completed the work in this department begun by Saunders in 1806, we owe a debt of gratitude. In 1860 he gave to the world his celebrated work *On the Diseases of the Ear*, which is mainly a record of twenty years' work on the pathology and physiology of the apparatus of hearing, and of other discoveries in the science of otology. It is this work, and the immense influence that his teaching exerted during his lifetime, and which has been even strengthened since his death by the tradition of his labours, that entitles him to be called the *founder* of aural pathology.

To Wilde of Dublin, also, we owe much; for he, by accurate clinical study and observation of the diseases of the ear, sought to place, and indeed succeeded in placing, aural therapeutics on a rational, and, therefore, on a sure foundation of well established facts.

It was this work, so ably begun by Wilde, that Hinton completed in his book, entitled *The Questions of Aural Surgery*, and *An Atlas of the Membrana Tympani*; works which, in the future years, will "serve as stepping stones for others to rise upon."*

In Germany, also, there has been a noble band of workers in this special field of inquiry, foremost among them being von Tröltsch, Schwartze, Politzer, Moos, Wendt (now alas dead), Gruber, Voltolini, and other less notable names, have done good and lasting work as special pathologists in the domain of ear diseases.

To the results of the labours of these great men we succeed, and no one can deny the richness of the inheritance; men, by the way, whose influence and whose teaching have made the speciality of aural surgery respected wherever it is practised.

Our study of aural pathology is the study of the symptoms of the diseases of the ear, and of the tissues, and organs, and structures in contiguity to the ear; hence called by the name of *aural pathology*.

It includes:—

(*a*.) *A general survey* of the anatomy and physiology of the apparatus of hearing.

(*b*.) *A general survey* of the etiology and the evolution of the diseases of the ear—*i.e.*, their natural history.

(*c*.) *A general survey* of the subjective symptoms of these diseases in relation to their causation, and to the tissue changes that accompany them.

(*d*.) *A general survey* of the direct consequences of primary ear disease in respect to other organs and parts of the body.

(*e*.) *A survey* of the general pathological states that cause tissue changes in the ear.

(*f*.) *A particular* consideration of the *objective facts* of the special pathology of the ear in relation to their clinical histories, and especially to their diagnosis and treatment, so far as can be accomplished, or is desirable.

Under this latter division of our study we shall consider the following:—

---

* Sir William Gull in the preface to Hinton's *Life and Letters*.

I. How to make a *post-mortem* examination of the ear.
II. The pathology of the temporal bone in general.
III. The diseases of the auricle and of
IV. The external meatus auditorius.
V. The membrana tympani.
VI. The tympanum and its contents.
VII. The Eustachian tube and its muscles.
VIII. The processus mastoideus.
IX. The labyrinth and its several parts—viz., the *vestibule*, the *semi-circular canals*, and the *cochlea*, and their contents.

Let us now consider the healthy ear from an anatomical as well as a physiological and physical point of view; in other words, let us examine briefly the general anatomical form and structure of the apparatus of hearing, and the functions of its several parts.

Since the time of Valsalva, who flourished as an anatomist in the seventeenth century, and who devoted fifteen years of his life to the study of the anatomical structure of the organ of hearing, till now, the ear has been divided by anatomists into three parts, named respectively an *outer*, *middle*, and an *inner* ear, or part.

Physiologists, on the other hand, have divided it into a *sound receiving* and a *sound perceiving* organ—the former. or sound receiving portion of the organ, comprising the outer and the middle parts of the ear, as it is divided by the anatomists, while the inner ear of the anatomists becomes the sound perceiving part of the organ, as it is regarded by the physiologist.

For the most part, the whole of the apparatus of hearing lies deeply situated in the head, and thus is protected, in a large measure, from injury, while at the same time it is equally well hid from inspection; indeed, one part of the organ—the labyrinth, is quite beyond the reach of ocular examination.

Throughout its entire extent, the organ of hearing exhibits a markedly spiral form, a fact which is most manifest in the semi-circular canals, and in the cochlea.

But this fact of form is also quite as pronounced in other parts of the ear, in witness whereof we may instance the spiral direction that the ossicular chain takes in its passage across the tympanic cavity, from the inner surface of the membrana tympani to the fenestra ovalis on the inner wall of that cavity; and to the decidedly spiral course of the external auditory canal we may also point, the inner end of which is closed by the membrana tympani, and which last named structure is itself inserted into a spiral bony frame called the annulus tympanicus. The curves, also, that mark the anterior surface of the auricle, are spiral in form, and though less markedly so than the other parts of the organ which I have just named, they are also illustrative of this fact of form of the hearing apparatus.

Attention was first called to this special form of the human ear by the late James Hinton,* who regarded it as of some importance in relation to the physiological acoustics of the apparatus of hearing, an opinion which seems quite worthy of further consideration from experimental physiologists, on the grounds that all motion, in passing through resisting media, tends to take the spiral direction, of which fact there are numerous instances in proof; as for example, the passage of a stone, or that of an air bubble through a jar filled with water, or the spiral form that smoke assumes when it is driven with some degree of force through a narrow opening into an apartment, the atmosphere of which is quite still, or at least not much disturbed by currents; a familiar example of which is sometimes seen in the smoke that is puffed from a tobacco smoker's mouth into the open air, when it is still and calm.

I have told you how the ear is regarded by the anatomist and by the physiologist, and now let us look at it from the physicist's point of view.

To him the ear appears to be a delicately constructed pneumatic machine, admirably adapted by its form to collect, to transmit, and to record the vibrations that arise in the surrounding air by the motions of matter. He looks upon the outer ear as a curiously constructed arrangement, formed funnel-like, for the purpose of collecting and of transmitting these sound-waves to the "drumhead," as the membrana tympani is popularly called, which vibrates in consequence. These vibrations of the "drum skin," in themselves being conveyed across the "drum" cavity by the bridge-like chain of hearing bones, and which in turn transmit their vibrations to the membrane of the oval window. From this tiny membrane, whose almost invisible motions are caused by the equally invisible motion of a tiny piston, as the stapes bone may be called, these vibrations are next conveyed to the fluid of the labyrinth, where, by a peculiar arrangement of the structures of its several chambers, they are there not only intensified, but also differentiated in their nature and quality, and, at the same time, are recorded with a never failing accuracy, within a range of from 16 to 40,000 vibrations in a second, whether they arrive at the recording part of the instrument in succession or simultaneously. He regards it also as a machine which is prone to disturbances in the air-tension of its principal cavities, but which disturbances are provided for by an admirable and highly sensitive adjustment apparatus, whereby the evil consequences of these derangements in its air-tension are, under ordinary circumstances, averted.

---

* In a paper on "Physical Morphology" in the *Brit. and For. Med. Chir. Rev.*, 1858, and since reprinted in his book, *Life in Nature.*

Such, then, is the organ of hearing, viewed from an anatomical, a physiological, and a physical standpoint. But as our aim is to know the ear as a healthy organ and with reference to its pathology, we are concerned chiefly with it in regard to its structure and to its function, and less in regard to the physicist's view of it, although even this latter cannot be altogether overlooked.

ON A CASE OF COMPLICATED MALFORMATIONS.

By JOHN KEITH ROBERTSON, M.D., Greenock.

In May, 1882, I attended at the accouchement of Mrs. ——, who was naturally, speedily, and safely delivered of a male child. It was her third confinement. She is tall, well formed, and healthy. In about half-an-hour from the time of my arrival, the strong expulsive pains sent the head into the world, and I expected the shoulders sooner than they came. But after they cleared the vulva, I was surprised to find that the body did not follow. Thinking the delay likely to endanger the respiratory chances of the child, I assisted expulsion by gently pulling the body. When the body was free, the cause of the detention was very apparent, in the shape of a very large bag, bulkier than the child's head itself, filled with cerebro-spinal fluid, constituting that tumour so often associated with spina bifida. The child's face was livid—quite black, indeed; signs of life were long of coming, but he eventually came to, and lived till Sunday afternoon, the 28th.

The father is a tall, muscular, healthy man, and the two former children are healthy, and quite *au fait* as to bodily perfection of formation. This unfortunate infant had a second congenital malformation in the shape of talipes varus of both feet, to an extreme degree. A third congenital malformation consisted in the absence of the anus; and a fourth in the absence of the scrotum, and of the skin covering of the penis.

The nurse, an old and experienced one, asked privately whether it should be considered a boy or a girl. The apparent absence, through non-descent, of the testes, combined with the absence of the scrotum, left a vulval-shaped appearance of the pubes, and naturally enough suggested the question. The parts not covered by skin being raw and exuding blood, which stained the napkins, had a strawberry-red colour.

I had intended to try to get the appearances photographed, but, not calling on the 27th, on the 28th I found the dead infant coffined.

The penis was the channel of exit of the fæces as observed during the act of defæcation. The form of the alvine evacuations corresponded to the tube of the penis through which they passed, and consisted of small, roundish, flake-like droppings—a few together, flattened like rouleaux, of a putty-like consistency, and whitish-yellow colour. The penis itself, by lack of skin coverings, was exposed to its root, and therefore appeared longer and thinner than one covered by skin, where not only the skin of the organ itself tends to its thickening, but the reflection of the skin of the organ on to the pendulous scrotum beneath, covers up the root parts of the penis. There being no glans penis, and no prepuce, it tapered gradually and gently to its point, which, at birth, curved sharply round, leavings its tip pendulous, so that the urethral orifice looked downwards. The pendulous part seemed to retract afterwards, so that the curve was much modified and the penis straighter. At birth the tip was quite black, and suggested gangrenous action there; but the blackness wore off, and I now attribute its appearance to the adherence of meconium on the part.

The child urinated, and though at first it was difficult to say whence the urine issued, and impossible to define exactly the urinary tract, still it was luckily observed to trickle down from a furrow or fold of the perineal skin, below, or posterior to, the root of the penis. It came from some opening or aperture so diminutive as to escape observation, about half-an-inch above, or anterior to, the normal situation of the anus. It trickled downwards perplexingly, totally deficient of that force which could cause a stream, and indicate the aperture. The urinary tract seemed quite disconnected with the penis, from which latter no urine was at any time observed to escape.

The urinary and fæcal apertures of exit were thus reversed as to position and organ, the structure which develops into the anal aperture in ordinary cases being solely the urinary aperture in this case, and that which is normally the urinary exit canal being here the only anal aperture, or at least the only channel whence the fæces escaped. Strictly speaking, of course, the place of exit of the urine in this case does not develop into the anal aperture, which is posterior to that position. But in the early development of these parts, the uro-genital sinus and the termination of the intestinal canal come together at that part, and together have a common opening for the exit of urine and fæces. I look upon the aperture of exit of the urine in this case as the prolongation externally of the sinus uro-genitalis of the early fœtal state.

Absence of the anus was a marked feature in this singular case of malformation; in all probability the rectum was absent also. The absence of any indication whatever of the situation of the anus, whether by depression, thinness, or discoloration of the skin of that region, would lead us to this conclusion, in conjunction with the channel of exit of the fæces.

At the root of the penis there was a curious appendage coming off on the inferior aspect, consisting of two small flesh-like cornua—one on each side, reaching out laterally. The penis itself inclined to the left side of the child, and always kept that position; consequently the cornu of the right side appeared larger. They seemed to possess the identical structure and red colour of the penis, of which they formed a part. What might they be?—the glands of Cowper? Their situation would, in my opinion, correspond thereto. But the absence of glandular structure, to appearance, negatived that idea. For a similar reason I abandoned the idea of their being parts of what anatomists describe as the anterior prostate, or ante-prostatic gland. They did not fit in exactly to the described position of the anterior prostate, which latter is said to lie in front of, and between, Cowper's glands. The position and appearance of these cornua indicated that they were likely those occasional appendages of the bulbous portion of the penis, termed "the lateral bulbs of the bulbous portion," and exposed thus, as the other parts of the spongy and cavernous portions, by the absence of skin.

During the few days the infant lived, the oozing of blood from the parts without cuticle became entirely staunched. That part of the vulva-like orbit, the skinless testicle portion on each side, had all the appearance of skin formation, passing from crimson-red to bluish appearance, as one observes on the surface of an ulcer in the early process of skin formation. Had life continued, I do not doubt that skin covering would have been duly effected there, and extended over the penis also, though no such skin formation was attempted in that organ in the time. If so, when the whole had become covered by skin, and when, in further time, the testicles had descended, carrying the skin before them to form the scrotum, the appearances would likely have improved so much as to become quite passable.

A part above the pubes was not covered by the skin; it was at birth of smooth and white bloodless appearance, quite like the funis itself, which latter was inserted into its superior border; it extended down to the symphysis pubis, and was of a somewhat triangular shape. In colour it afterwards changed to yellow; it then had the appearance as if it were the thinned skin through which a great abscess was about to burst. Superiorly it terminated by a rounded crescentic border, which extended only about a third of an inch above the navel, embracing the funis in its upper concavity. It no doubt represented the boundaries of the urinary bladder, and showed an approach to that state of the organ where, through entire absence of continuity of the abdominal parietes, the viscus becomes extroverted.

The enormously distended sac of cerebro-spinal fluid showed no sign of thinning or discoloration of its firm skin covering. The presence and nature of this tumour constituted the main *causa morbi et mortis* in the case, though the want of a proper exit for the fæces was itself sufficiently dangerous to life. The mode of death was what one expects in such cases of spina bifida—spasmodic twitchings and startings, with occasional sharp cries, with gradual overpowering of nervous ability till it is at last inhibited at its central source and origin.

Of course, the interest in this case lies in its abnormalities alone. The healing art was impotent here. The infant was emphatically "born to die." In the race of "the survival of the fittest" it was incapable of running. It was also a case in which no one grudged the intervention of the friendly offices of kindly death, to bring relief to all concerned.

Anatomists explain some of these abnormalities as persisting conditions of parts as they normally exist in the early stages of fœtal development. They say they are the results of the arrest of development at the stages in which we find them; and that further differential development in normal circumstances gives us normal conditions and structures. Thus embryologists have given the "reason why" for many a *lusus naturæ.*

In this case, though we had not the state of matters answering to their description of the "common cloaca' of the early weeks of utero-gestation, inasmuch as there was no anus, and probably no rectum, there being also the fact of communication between the bowel and the urinary tract, whether in the bladder or urethra not being known; still it may afford us a clue to the probable reason why the urine found an exit at the particular spot it did. In absence of the "common cloaca" the "single and private cloaca" for the exit of urine remained persistent, if the view be correct that it was the opening externally of the sinus uro-genitalis of the same stage of development.

I do not know that embryology can so account for the various vagaries observable in the lower part of the bowel and its termination, as for imperfect, occluded, imperforate, or absent anus or rectum; or for the unusual terminations of the bowel in the bladder or urethra; or for its ending in a *cul-de-sac,* with no communication externally. But doubtless there are laws which regulate these conditions, the knowledge of which would afford a key to such abnormalities, and show unity of design and conformation in the structure of the human system, a universal design, to which these abnormalities are not even exceptions. Exceptions in human institutions are justly said to prove the rule. But our Creator does not work by rule, but by law. And if we choose to call these abnormalities exceptions, then they are exceptions, not to design, or to uniformity of design, but to the completion, or perfect consummation, of the working out of that design.

Comparative anatomists have taught us that many of these abnormalities are representative of the normal and usual states of parts in the normal and fully developed condition of some of the lower animals: the *ne plus ultra* or perfection of their conformation. Thus, they inform us that this common cloaca state of early fœtal development is the perfectly developed state in fish and in fowls. It seems quite legitimate to argue therefrom the superiority of development in the human system, and also that there is a general type of animal construction, considered as a unity, in the animal, and particularly in the vertebrate animal kingdom. And most interesting and instructive must such studies of evolution become.

But, to rush to the conclusion that, because there is an evident general type of animal construction, and that lower types which remain persistent, and have analogues in more highly developed and differentiated types, there is a capability, or even a tendency, to spontaneous natural progression from lower to higher types in gradation, through epochs of time, seems altogether the gratuitous assumption of romantic spirits, and perhaps finds its fitting poetical parallel in the belief in the "Transmigration of Souls," as has been aptly remarked. It is not exclusively by observing the *modus operandi* that one judges of the worth or utility of the finished article. But the utility and worth of the finished article may declare and justify the design and aim of the Maker in making it. If God has made man to honour, as the potter the clay, it is surely a gratuitous assumption to suppose that inferior animals, formed to comparative dishonour, were either intended for, or endowed with, potentialities fitting their final development into the same state and honour. Such an assumption is not only gratuitous and wholly incapable of proof, whether we search the pages of history or peer through the dim glass of palaeontology, but it is also humiliating to man. And while this evidence does not amount to irrefutable proof, let an appeal be made to our common innate consciousness, and see whether there is not in those inner depths of our being enough to raise a revolt against such earth-born materialism.

While the onus of proof rests with those who raise the theory, we are far from want of sympathy with the difficulties of the study; further still, we hope, from undervaluing or from deprecating such a legitimate and worthy branch of enquiry, and farthest of all, we fondly trust, from that bane of all progress in enlightenment, a too prejudiced and fixed adherence to old traditions.

Meantime, we must acknowledge that, to our minds, branchial arches and hand-shaped feet, and human-approaching brain and convolutions, do not express evolution from frogs, *via* tadpoles, fish, and apes, as a fact, but merely as an unsettled theory. We think rather we find fresh and new *creations*, not invariably higher in the animal scale, in the stony records of geology: not evolution.

Looking to the finished workmanship, so to express it, the crown and glory of all God's creation, granting that man conforms in structure to the general type of animal creation, it is possible that evolutionists may have erred in viewing too much the typication—the *modus operandi*—of the structure of our animal frame, and too little the evident superiority of the completed structure—the complicated being of body, soul, and spirit—man. And though in human conduct the end does not always justify the means, it would be surely worse than presumption in the creature to say that the end was less perfect, because he traced, or thought he could trace, the means by which the end was achieved in the Creator's type of workmanship. But, apart from theoretical discussion, it is something to be able to give practical consolation to distressed parents, in cases of malformed children, in pointing out that there are scientific modes of accounting for such malformations, little and inadequate as such consolation may be.

NOTE ON INCREASED RESISTANCE ON PERCUSSION AS AN AID TO DIAGNOSIS.

By JOHN S. MAIN, M.D.

I have been much struck and pleased for some time back by the valuable aid we derive from increased resistance, as felt by the sense of touch through the pleximeter finger, in the diagnosis of disease, and the "mapping out" of organs. On referring to several text-books, I hardly think due justice is given to this physical sign, and so I trust the following remarks may not be amiss.

In phthisis pulmonalis I take this symptom next to some alteration in the respiratory murmur (especially abnormally prolonged expiration), to be the most valuable physical aid we possess for its *early* detection: much more valuable in the majority of cases than any alteration in the quality of the percussion note.

To derive full benefit from it, however, it is well to bear in mind, that according as the deposition of tubercle is superficial or deep, so will the stroke given to the finger require to be the slightest touch, or a more definite tap: just as the same rule holds true for properly bringing out the percussion note.

In all cases, therefore, it is well to begin with a feeble stroke, gradually increasing in strength till we have gained satisfaction.

With the aid of this sign alone, it is often possible to form a pretty accurate idea as to the presence of tubercle; but taken in conjunction with the stethoscopic signs, and those derived from the sense of hearing as obtained through the pleximeter, it is simply invaluable. In advanced phthisis, I need hardly add, that it is a symptom never absent.

Again, in the "mapping out" of the outlines of the solid organs on the chest or abdominal wall, also as a means of detecting the limit of dulness in pneumonia and pleuritic effusion, I have found this aid very valuable. Indeed, taken with the quality of the percussion note, the cardiac, hepatic, and splenic areas, can be marked off with the greatest precision.

May I not add, that the double aid to diagnosis we thus derive through the pleximeter finger, points strongly to the superiority of those pleximeters with which nature has provided us, to those sold at the instrument makers.

VOLUNTARY LOCK HOSPITALS AND THE CONTAGIOUS DISEASES ACTS. REPLY TO DR. ALEXANDER PATTERSON.

By FREDERICK W. LOWNDES, M.R.C.S. Eng.,
Surgeon to the Liverpool Lock Hospital.

In the *Glasgow Medical Journal* for December, 1882, is a paper by Dr. Alexander Patterson, in which he endeavours to show the advantages of voluntary Lock hospitals, and of the Glasgow Police Act, over the compulsory provisions of the Contagious Diseases Acts. I also was summoned to appear before the Select Committee of the House of Commons, the object of my evidence being to show the deficiencies of the voluntary Lock hospital system, and the advantages which might be expected to accrue from compulsory powers. I also wished to bear testimony to the excellent working of the Contagious Diseases Acts, as observed by me on visiting Aldershot, Chatham, Devonport, and other districts in which those Acts are in force. Believing that my views express those of many readers of the *Glasgow Medical Journal*, and bearing in mind the excellent precept, *audi alteram partem*, I have ventured to reply to the various remarks contained in Dr. Patterson's paper. While willingly accepting many of his facts, I cannot agree with the inferences he deduces from them.

For instance, he assumes that, because the number of patients in the Glasgow Lock Hospital has been decreasing while the population has been increasing, that therefore there has been a decrease of prostitution and disease. This is *post hoc ergo propter hoc* with a vengeance. In other words, we are asked to believe that, in the whole city of Glasgow and its suburbs, there are not to be found sixty females suffering from venereal disease who might be induced to enter the hospital! With every respect to Dr. Patterson, such an assumption is, to my thinking, utterly contrary to all reason and common sense. Mr. M'Call was most careful to admit in his evidence before the Select Committee that the Police Acts only applied to the City of Glasgow and not to the suburbs beyond; hence, there is no evidence that the number of prostitutes, admitted or clandestine, outside the city boundary has been reduced. To ask why they do not come, as Dr. Patterson does, is simply begging the question. In Portsmouth and Plymouth voluntary Lock hospitals, each containing about thirty beds, were tried by the Government some years before the Act of 1864 was passed. But even this small number of beds was more than enough, and they were rarely filled. That this was no proof of any decrease of disease was manifest after the Act of 1866 was passed, when 162 beds in the Royal Albert Hospital at Devonport, and 120 beds in the Royal Portsmouth, Portsea, and Gosport Hospital were all filled with females suffering from venereal diseases. Again, in the Winchester Infirmary, six beds were formerly set apart for females with venereal diseases, and because they were generally empty, it was rashly concluded that they were not required. But when, in 1870, Winchester was placed under the Acts, out of a total of one hundred and fifty-six women no fewer than sixty-two were found to be diseased, or enough to have filled up the six beds just ten times over.

When I was first appointed to the Liverpool Lock Hospital in 1875, the number of females admitted was in that year only 172, while in 1876 it fell to 150, and in 1877 to 141. It would have been highly gratifying to have referred this decrease to the decrease of prostitution and disease, especially as the local police had, in 1871, under the Vagrant Act, arrested large numbers of prostitutes for soliciting in the streets, and for being drunk and disorderly. But as I knew perfectly well that such an assumption would have been delusive, I set to work to find out the real cause of this decrease. Up to the close of 1876, students of the third and fourth years were permitted to see the examinations of females, when this was abolished on the suggestion of my then colleague, Mr. M'Cheane (now consulting surgeon to the hospital), and myself. We also suggested that the patients should not be required to scrub the floors, as hitherto had been the case. Moreover, in company of one of our chief superintendents of police and other police officials, I visited a number of brothels, saw the inmates, and conversed with them. I found that the hospital was perfectly well known, and fully appreciated by such as had been inmates. They were very reluctant to admit that they themselves ever suffered from disease, though willing enough to volunteer information as to other women. The result of all this will be seen in the following figures:—

Year.	Number of Females Admitted.
1878,	183
1879,	215
1880,	291
1881,	280

My present colleague, Dr. Bernard, and I, have every reason to expect a still larger number this year—not because disease has increased, but because the hospital is better appreciated. We have only 25 beds for females, and hope that in process of time the committee will be encouraged to increase the number. Admission is free, and we have no power to detain any who wish to leave. Dr. Patterson does not make any allusion as to the state of the women on their admission—whether they are severely diseased or not. Ours are in such a state, that I am constantly saying—"Why on earth did you not come here before?" and Dr. Rawdon Macnamara of Dublin, in his evidence, spoke even more strongly of the condition of many of the women admitted into the Westmoreland Lock Hospital. Similar testimony was given by Mr. James Lane as to the state of many of the females admitted into the voluntary wards of the London Lock Hospital. On the other hand, the patients admitted into the Government hospitals are almost invariably suffering from mild forms of the disease, the only exceptions being in the cases of women who have just come from unprotected districts, and who are most severely diseased. This is the reason why those who support these Acts urge the importance of periodical medical examination, and compulsory admission to hospital at once of all women found to be diseased—not to prevent men from the consequences of vicious indulgence, but to save these unfortunate women themselves from untold suffering.

While perfectly agreeing with Dr. Patterson that "females however abandoned, rarely altogether lose the sense of shame," I fail to see how the Contagious Diseases Acts causes the "last remnant of modesty to be driven away." A woman enters the examining room in which are only the visiting surgeon and the female attendant, generally a nurse. From what I have seen of the visiting surgeons, Dr. Barr of Aldershot, Dr. Jardine of Chatham, Mr. Pearl of Windsor, Dr. Archer of Devonport, and Mr. Chaplin of Kildare, I am perfectly satisfied that the examinations are conducted by them with every kindness, delicacy, and care. No one except the nurse is ever present, and among the other "instructions for visiting surgeons" is this: "every examination is to take place in the presence of a female attendant or nurse, and is to be completed with as much regard to delicacy and the feelings of the woman examined, as in any case of private practice." Surely Dr. Patterson must see that, so far as the examination itself is concerned, the Acts do no more to drive away the last remnant of modesty than he and his colleague, or my colleague and I, who conduct examinations precisely in the same manner. We never find it necessary to administer chloroform in order to pass the speculum, as we should, in "cases of young girls with acute inflammation of the parts," prefer to wait until the inflammation had subsided, when the speculum could be passed without pain. My experience, as I stated before the Select Committee, is that many a woman has learnt delicacy in our examining room for the first time for many years, and the sense of shame has been recalled to her by a few kind words from my colleague or me. Such is also the experience of all the visiting surgeons I have met and conversed with.

We cannot detain patients in our hospitals till cured, a circumstance which is much to be lamented. If any patient wishes to return home to her friends, or go to a "refuge," or enter a situation, it is obviously better that she should go there thoroughly cured. If, on the other hand, she will, in spite of "kindly advice," or "after being gently and kindly reasoned with," persist in returning to her former vicious life, surely the desirability of her being cured is still more obvious. For how vain have been all our efforts, and what are the results? She has been simply kept in hospital for a time to be sent out even more dangerous than when she came in. Then she was so diseased as to be unable any longer to ply her trade. Now she is sufficiently well to return to it without inconvenience to herself, and sufficiently diseased to inflict it upon the opposite sex to an extent of which neither Dr. Patterson, nor I, nor any one, can form the faintest idea. Our rules at the Liverpool Lock Hospital require us to refuse admission to any former patient who has left uncured, a rule which is not so salutary as I could wish, though it is a necessary one. But its results show very clearly that this freedom given to women to come into hospital after they have done all the mischief of which they are capable, and letting them out to repeat the same mischief is the weak part of voluntary Lock hospitals, and is probably one reason why they enjoy so little of the public support lavishly bestowed upon all other hospitals, general as well as special.

Dr. Patterson considers that compulsory admission and detention of girls in hospital is most unjust, because it is practically imprisoning them for suffering from disease inflicted upon them by the opposite sex. Is Dr. Patterson aware of what was stated by Dr. Lyon Playfair in his admirable speech, delivered in the House of Commons when the first motion to repeal these Acts was debated in 1870? Remarking on the numerous petitions against the Acts that emanated from Scotland, principally on the ground of interference with the liberty of the subject, he reminds his countrymen that the credit or discredit of having found out how to arrest these contagious diseases rests with them. "In the year 1497, the Aberdeen magistrates shrewdly suspected that the disease was not an epidemic, and they ordered all diseased women to abandon their evil courses, and shut themselves up in their houses till they were cured, branding on the cheek with a red hot key those who came out too soon! And six months later the Privy Council ordered the magistrates and Town Council of Edinburgh to collect all diseased women, along with their doctors! on the sands of Leith, boats being provided to transport them to the island of Inchkeith, where they were to remain till cured, on pain of being branded in a like way." Well might the right honourable gentleman add, "Really the North should not be so hard on its gentler Southern progeny." Since 1867, all persons of either sex, suffering from any contagious disease while inmates of a workhouse, are liable to be detained till cured, and this has been stated, on legal authority, to apply to venereal diseases. The knowledge that such a power is possessed induces all such persons to remain till cured, and no one complains of it as being an injustice. It seems to me unfair and unjust to lay the blame of these diseases entirely upon our sex, as Dr. Patterson does, when he implies that it is all the fault of men going after women. Is this so? Do not women go after men even to the extent of what Canon Puckle, of Dover, described as "assaulting prostitution." Place a camp where you will, at Chatham, Aldershot, or the Curragh, the soldiers will be soon surrounded by a number of these women far more than sufficient to satisfy the demands of the vicious ones. And were a camp to be established on Ailsa Craig, or the Bass Rock, I have no doubt whatever that these women would make their way to them even there. I maintain, therefore, that women or girls persisting in a life of prostitution after they are found to be diseased, are not unjustly treated when they are forced into hospital and detained there till cured. While I sympathise most sincerely with these unfortunate women, and have no wish whatever to shield men from blame, I ask in return for some sympathy for the merchant seaman who is sent on a long voyage with a disease inflicted upon him by the opposite sex, and who will be for months without medical treatment. Five years ago I received a letter from a Dr. Patterson, of Constantinople, of which I give an extract:—

"I visited a small screw steamer a short time since at the request of the captain, to examine his crew. I found the steward with two large suppurating buboes of six weeks' standing. The condition of the wounds and the dressings were perfectly filthy and stinking. The cook was covered with secondary syphilitic eruption and ulcerated sore throat. Two men had large syphilitic ulcerations, one with gonorrhœa. Now every man was as dirty as he well could be. All had shipped with the disease on them, and carefully concealed their state from the captain. Of course they performed their duties imperfectly, and the others of the crew as a consequence were overworked. This is only one of numerous instances of a similar character. Men are daily admitted into hospital suffering from various forms of syphilis, most frequently chronic, and in such a state of disease and dirt that anything communicable must be communicated."

Seamen, both naval and mercantile, are looked upon by these women as their lawful prey, and in spite of all that has been done of late years to ameliorate the condition of merchant seamen, afloat and ashore, venereal diseases are still very prevalent among them. This strengthens the opinion held by those who support these Acts, that repressive measures which take no cognisance of disease (such as those in force at Glasgow), will leave a great evil uncured.

As regards the occupation of the female patients, on which Dr. Patterson lays so much stress, I fail to see its import. It matters little what they call themselves so long as they are to all intents and purposes prostitutes. If prostitution is to be reduced to a minimum it must be by calling a spade a spade, and a prostitute a prostitute. A woman can always avoid "branding with the broad arrow," and "avowing herself a prostitute by Act of Parliament," by the simple process of not becoming a prostitute at all, either as a trade, or "as a means of augmenting her very small wages."

I can confirm Dr. Patterson's experience as to the freedom of Jews from venereal disease, and of the rarity of a Jewess as an inmate of a Lock hospital. While admitting the general morality of Hebrews, I cannot overlook two facts in connection with them. One is, that a Jewess who loses her virtue, loses also her nationality and her religion. The other is, that circumcision is undoubtedly a powerful factor in preventing venereal disease.

With regard to the ages of those whom Dr. Patterson calls the "Queen's women," as compared with those of the Glasgow Lock Hospital patients, it would have been better had he quoted the whole of the paragraph from the Report of the Select Committee, of which he has only given a part. The paragraph in its entirety is as follows:—

"Attention was called to the fact that some women remain on the register up to a more advanced age than formerly. This is attributable, not to the so-called hardening effect of the Acts, which have proved powerful for purposes of reclamation, but to the improved health of the women and their freedom from neglected disease, which in former times cut short not only the career but the life of the prostitute at an early age."

This, which to Dr. Patterson is so extraordinary a statement, is perfectly consistent with the facts alluded to in another paragraph of the Committee's report—

"Upon the third point raised by the advocates of the system, there seems scarcely room for serious doubt. Before the passing of the Acts, women of this class were sunk in a state of disease and misery which baffles description. They are represented as living like 'wild beasts,' in woods and drains without shelter, and almost without clothing, and without the slightest regard to or conception of ordinary decency. To speak of persons in such a bodily plight as capable of moral reclamation would seem little short of an absurdity; and it is clear that so far as the Acts have tended to improve the physical state of fallen women (and of this fact your Committee have had abundant proofs), they must of necessity have helped to place them under conditions in which their moral reclamation becomes for the first time possible."

It is unfair to compare the prostitutes of Glasgow with those just described, their relative conditions being totally different. One fact Dr. Patterson has proved, viz., that there is evidently much juvenile prostitution in Glasgow, whereas in the districts where the Acts are in force it is completely eradicated.

The quotation from John Milton applies to Glasgow and its Police Act quite as much as to the Contagious Diseases Acts. That the law in Glasgow has not utterly taken away the sin is manifest by the fact that the chief constable admitted the existence of 38 brothels, and I have yet to learn that they have been suppressed. The Acts do not recognise prostitution as a necessary evil, though they do recognise, as the Glasgow Police Act does, that prostitution is an existing fact. That the Acts are not a "distinct state recognition and licensing of vice" is proved by the 12th clause of the Act of 1866, which enacts that—

"No hospital, however, can be certified under this Act, unless at the time of the granting of a certificate adequate provision is made for the moral and religious instruction of the women detained therein under the Act; and if at any subsequent time it appears that in any such hospital adequate provision for that purpose is not made, the certificate is to be withdrawn."

That this is no dead letter is shown by the Committee's report—

"Your Committee are satisfied, on the evidence placed before them, that the moral and religious influences brought to bear in the certified hospitals are largely productive of reclamation. Some witnesses spoke of the registered women who became patients as altogether hardened and unwilling to submit to good influences. The Rev. Flavel Cook, Chaplain of the London Lock Hospital, is of this opinion. But his views were controverted by Mr. Seton Karr, vice-chairman of the Hospital, who was able to point to a large number of registered women, patients in the institution, who were reclaimed through its agency. Your Committee desire to call special attention to the statements on this subject of Miss Webb, the Lady Superintendent of the Chatham Lock Hospital, and to the numerous testimonies which she produced as to the good effects of hospital detention upon the women under her charge, and the success of her efforts to bring them back to a virtuous and respectable life."

Dr. Patterson has given an extract from the *Minority Report of Proceedings of Select Committee* with reference to the police administration of Glasgow. Surely the *Majority Report* is entitled to at least equal respect, and yet Dr. Patterson has completely ignored it. It is as follows:—

"*Consideration of Evidence as to Reduction of Disease in Glasgow.*—Evidence was given showing a decrease in the number of admissions to the female Lock Hospital at Glasgow, and it was asserted that the system of police surveillance prevailing in that town had led to a considerable diminution of disease. Your Committee have observed that the administration of Glasgow aims mainly at the repression of public prostitution. It does not, like the Acts, provide any organisation by which women who choose to lead a life of prostitution privately, and without attracting public notice, can be brought under supervision. It is therefore impossible, in the case of a town circumstanced like Glasgow, to infer from the returns of a public Lock Hospital what the extent of disease may be among women who court privacy, and therefore do not enter such institutions. Besides, no satisfactory evidence was given as to the amount of disease amongst the male population, an element without which it is absolutely impossible to measure the real amount and intensity of venereal disease in any locality, or the success of any system for its diminution."

The quotation given by Dr. Patterson as to the result of the Acts, so far as females are concerned, also comes, I presume, from the Minority Report.

The alleged increase of disease is alluded to in the Report of Committee, which is as follows:—

"It is to be observed—(1) That the actual number of women who contract and propagate the disease is admittedly diminished. (2) That though the cases are more numerous in proportion to the diminished number of prostitutes, the disease is milder in form, and is arrested before it has become virulent, and inflicted the injuries which result from neglected contagion. (3) That notwithstanding the increase in the number of admissions to hospital among a given number of registered women in a given period, venereal disease among soldiers has diminished. This proves that hygienic purposes are better served by the presence of a reduced number of prostitutes, liable to increased risks of the recurrence of disease in a mild form, but subject to careful medical treatment, than by the presence of a large number of prostitutes in a chronic state of neglected disease."

Another paragraph seems to me extremely *à propos* of many of Dr. Patterson's statements, and is:—

"Your Committee also feel bound to observe that a very large proportion of the witnesses called by the opponents of the Acts admitted that they had never visited these places, or that they were very imperfectly acquainted with the administration of the Acts, and with the management of the certified hospitals."

Dr. Patterson's seven medical objections are soon disposed of:

1. The Acts are not directed against women, but against prostitutes—*i. e.*, against a class who, as stated in the Report of the Royal Commission (par. 60), "commit the sin of fornication as a matter of gain," and it may be added, in the deliberate exercise of their calling, and who are specially liable to contract and propagate disease in the exercise of that calling. Among the male part of the population no corresponding class exists, and it is difficult to see to what men such legislation would be applied. (*Report Select Committee*). If women only suffered as severely as men, there would be much less disease propagated by the former. To compare men and common prostitutes together as sources of disease appears to me still more manifestly absurd.

2, 3, and 4. These are all matters of opinion, the result of practical experience. An experienced examiner can have no difficulty in distinguishing the exact nature of the discharge, in detecting any "preparations," and most certainly in detecting the infecting sore if it be present. This was the conclusion of the Select Committee, who report:—"Your Committee, taking into account all the medical evidence given on this subject, have come to the conclusion that, while undoubtedly cases may occur in which the traces of disease escape notice, they are unusual, and that an experienced surgeon will rarely fail to detect venereal disease.

5. Mr. James Lane, of the London Lock Hospital, Dr. Barr of Aldershot, and other experienced examiners, consider the possibility of mediate contagion as very remote.

6. The probability of latent disease being undiscoverable, is only an argument in favour of examinations at more frequent periods.

7. The infecting power of the secretions of a syphilised woman has yet to be proved, and presents no difficulty to the periodical examination of prostitutes. If these are conducted carefully, and those found to be diseased are sent to Hospital, much disease is prevented, and the object of the Act is gained.

8. Abundant evidence was given before the Select Committee to the effect that the tendency of the Acts is to diminish clandestine as well as open prostitution.

Dr. Patterson attempts to show that the reduction of brothels has been greater in Glasgow than in the Devonport district, under Inspector Anniss. The comparison is not a fair one. The operation of the Acts comprises Plymouth, Stonehouse, Devonport, and the villages within 15 miles; also Dartmouth. Glasgow comprises, for the purposes of the Police Acts, the city only. Strong police measures applied to suppressing brothels are more likely to lead to clandestine prostitution than are the operations of the Acts; and the present number of brothels is quite inconclusive without the number in the suburbs. It is not a little remarkable that while Inspector Anniss' reduction of brothels in his district to 70 was strongly disputed by the opponents of the Acts, this reduction in Glasgow to 38 has been accepted without the least hesitation.

The laws in Paris which related to prostitution were so totally different to our Contagious Diseases Acts that it is quite incorrect to term the former so. Referring to this the Select Committee say:—

"It has been usual for the opponents of the system to assume that the Acts are analogous to certain continental methods of dealing with prostitution, which avowedly offer facilities for vice. It seems to your Committee that the contrast between the Acts and the notorious features of certain continental systems affords strong proof that the former violate no principle of morality. The English statutes make an attempt at reclamation by moral and religious agencies an essential part of every attempt to check the evils of prostitution. The foreign systems tend to confine public women in brothels, where every good influence is avowedly excluded. The insistence on religious and moral influences makes the system in principle an ally and not an enemy of religion and morality. Whether they practically work in this direction is a question of fact hereafter to be considered. But your Committee are of opinion that, so far as the objection of principle is concerned, it is not well founded."

The reference to Hong Kong has nothing to do with the Acts in England, as nothing in the shape of a license is issued in the protected districts. All certificates of freedom from disease are given to the police and not to the women. Dr. Patterson tells us, "In 1868 the second battalion 5th Fusiliers, 648 men, was stationed at Aldershot and Dover, half time at each place, and both protected districts, where 126 cases of venereal disease occurred. In 1870, the same battalion, 599 men, stationed in Glasgow and Ayr, had only 103 cases in the twelve months." From this statement Dr. Patterson desires to draw the inference that in Glasgow, with a voluntary Lock hospital, more favourable results were obtained in the 5th Fusiliers than at Aldershot and Dover under the operations of the Acts. But had he been more familiar with the facts and the progress the investigation of the subject has made within the last few years, he would have been aware that his premises do not establish his conclusions. It has been very clearly demonstrated that the frequency of venereal disease among a small body of men is subject to so many disturbing causes, that it cannot be employed by itself to establish any general conclusion. And even when very large bodies are under observation, the incidence of the disease varies so much from year to year, when measures are not taken to limit its spread, that no trustworthy results can be obtained by comparing the experience of one year at one station or set of stations, with that for another year at another set. Dr. Patterson has fallen into both these errors, but there are others also that must now be mentioned. Dover was *not* a protected station in 1868. It came under the operation of the Contagious Diseases Acts in January 1870 only, and though Aldershot was under the Act in 1868, it was not then carried out with the same strictness as subsequently. The figures given by Dr. Patterson, when converted into ratios in the usual manner, show that the 5th Fusiliers at Dover and Aldershot in 1868 had a ratio of 194 per 1,000 of venereal cases, while at Glasgow and Ayr in 1870 they had a ratio of 172 per 1,000, a reduction of 22, or 11 per cent of the former ratio. These high ratios indicate that Dr. Patterson has included gonorrhœa, as well as primary venereal sores, under the term "cases of venereal disease," an important fact, as will appear immediately. In the *Army Medical Department Report* for 1880, at page 368, there is a table showing the strength and admissions for primary venereal sores and gonorrhœa at twenty-eight stations each year, from 1860 onwards. Taking the data for Dover and Aldershot for 1868, it appears that at Dover, with a strength of 2,366 men, the admissions for primary venereal sores were 111 per 1,000, and for gonorrhœa 181, together, 292 per 1,000; and at Aldershot, with a strength of 9,879, the primary sores were 77 per 1,000, and gonorrhœa 100, together, 177 per 1,000. As the 5th Fusiliers were six months at each station, half the sum of the admissions at each station in the year represents the risk they were exposed to. Thus $\frac{292+177}{2}=234$, the ratio per 1,000 of cases that might have been expected in the battalion at these stations in 1868; the actual ratio which came under treatment was 194 per 1,000 only, or 17 per cent less than that prevalent at the station during the period. In 1870 the strength at Dover was 2,477, the admissions for primary sores 30 per 1,000, and for gonorrhœa 90, together 120 per 1,000. At Aldershot, with a strength of 11,235, the admissions for primary sores were 67 per 1,000 and for gonorrhœa 95, together, 162 per 1,000; and if the 5th Fusiliers be supposed to have passed six months at each station as in the previous instance, the admissions might have amounted to 141 per 1,000, being 31, or 19 per cent less than they were at Glasgow and Ayr during the same year. And had the battalion enjoyed the comparative immunity it did in 1868, this reduction might have reached from 8 to 10 per cent more. It is obvious that Dr. Patterson's contention is altogether erroneous.

As the expense of working the Contagious Diseases Acts amounts to £30,000, while the saving of men's time in hospital in the Army is 5·38 per 1,000 daily; and, as there are about 50,000 men at the protected stations, Dr. Patterson estimates that the expense is £110 per man. But here, as elsewhere, he omits much that he should have included in his calculations. The deterioration of constitution avoided by reduction in the frequency of the disease is an important element; the amount of benefit derived by the Navy is another; and the influence of the Acts upon the rate of mortality from syphilis among the civil population in the districts where they are in operation, are all to be taken into the account. The last is an interesting and important fact. Taking the second and fifth divisions of the Registrar-General, which embrace all the counties south of the Thames and Bristol Channel, in which eleven of the twelve stations where the Acts are in force in England are stationed, and comparing the deaths from syphilis among the civil population for the five years, 1865 to 1869, with those for the five years, 1875 to 1879, the latter are 15 per cent lower than the former, while in the rest of England (exclusive of London), instead of a decrease in that period, there has been a rise to about the same amount. With these items set off against Dr. Patterson's, the cost per individual benefited will be enormously reduced. And I think that the opinion of Cromwell (whose memorable advice to his soldiers, "Put your trust in Providence, but keep your powder dry," is so well known and often quoted) as to these Acts would most probably have been the reverse of Dr. Patterson's. While exhorting his soldiers to be virtuous, he would, I think, have preferred to locate them in protected districts.

In conclusion, I would wish to remark that, while Glasgow and Liverpool each has its Lock hospital, the seaports of Leith, Greenock. and Aberdeen in Scotland, and those of Hull, Cardiff, and Newcastle-on-Tyne in England, have none. The Glasgow Lock Hospital is, in fact, the only one in Scotland; those of London, Manchester, and Bristol are the only voluntary Lock hospitals, besides the one in Liverpool, in England, while the Westmoreland Lock Hospital of Dublin is the only one in Ireland. Omitting the latter, which is supported entirely by Government, though voluntary in its principle of admission and discharge of patients, all the voluntary Lock hospitals in the United Kingdom can be counted on the fingers of one hand! The Liverpool Lock Hospital exists as a part of the Royal Infirmary, otherwise I should greatly fear for its continuance. The London and Manchester Lock hospitals live on in a state of chronic bankruptcy; the Bristol hospital keeps sixteen beds, just supported; and I see from their last report that the managers of the Glasgow hospital were obliged to withdraw the sum of £180 from bank to pay current expenses. There is a Lock ward for females in the Edinburgh Royal Infirmary with sixteen beds, in Aberdeen one with eight beds. To suppose that these are sufficient is to insult common sense. In a pamphlet published by me lately, and reviewed in the *Glasgow Medical Journal* for September last, I have shown what Government has done in the way of providing Lock hospitals for females, which is as follows:—

	Year Opened.	No. of Beds.
Aldershot, ...	1865	100
Chatham, ...	1870	68
Colchester, ...	1869	25
Cork, ...	1869	46
Devonport, ...	1863–68	150
Kildare, ...	1869	40
London (Prince of Wales Wing),	1868	73
Portsmouth, ...	1857–58	100
Shorncliffe, ...	1868	24
Total, ...		626

Which could be increased to a total of 645.

In other words, the Government has by these Acts provided in the few years comprised between 1864 and 1870, and in eight districts, more hospitals, containing in the aggregate more beds, than all the voluntary efforts of more than a century and a quarter have been able to effect in the three kingdoms. I think that if Dr. Patterson and those who oppose these Acts would try to realise these facts, it might lessen the bitterness, and, I must add, unfairness with which these Acts have been assailed. I think that if the money spent in fruitless attempts to effect their repeal, which sum was stated by one of their own witnesses to be £3,000 a year, were used in assisting the existing Lock hospitals, and providing new ones where they are most required, it would be spent more profitably. And to those who are disposed to pin their faith upon the repressive measures at present adopted in Glasgow, I would repeat the admirable words of Sir Henry Thompson, published fifteen years ago:—"I know of one way only on which we can rely for checking the disease; it is not by hunting down the prostitute, and chasing her from one retreat to another—a treatment which increases her recklessness and propagates contagion—but, on the contrary, by caring for her health and regulating her habits."

REVIEWS.

The International Encyclopædia of Surgery. Edited by JOHN ASHHURST, Jr., M.D., Professor of Clinical Surgery in the University of Pennsylvania, in six volumes. Volume II. Macmillan & Co., London. 1882.

THE Second Volume of this copious compilation is not long in following the issue of the First. Like its predecessor, it is composed of numerous articles, chiefly by American authors, the only non-American contributors being three Englishmen—Thomas Bryant, Watson Cheyne, and Howard Marsh. We shall adopt a similar method of review to that employed in the First Volume, giving a brief reference to the various articles separately.

The opening article, upon Contusions, by Hunter M'Guire, M.D., Emeritus Professor of Surgery in the Medical College of Virginia, Richmond, though brief, is a needless addition to and subdivision of a subject adequately treated of in the article upon wounds in general, which immediately follows.

Wounds, by Thomas Bryant, F.R.C.S., Surgeon to and Lecturer on Surgery, Guy's Hospital, London. One or two points are worthy of notice in the article by this experienced surgeon. Foremost is his attack upon and disbelief in the Germ Theory. In support of his opposition, Mr. Bryant adduces facts which, so far as we can see, have no bearing whatever upon the question. His arguments, to begin with, are based upon a false premise. Thus, to quote his own words:—"Attempts have been made to assign to the presence of germs every evil influence, and to regard these as the cause of inflammation and suppuration in every open wound;" and then follow examples to show that in tenotomy, where air is excluded, inflammation and suppuration may take place, and in fractured ribs with emphysema there may be no inflammation and suppuration. These illustrations may support his own allegation, but in no way affect the true intent of the Germ Theory.

Inflammation and suppuration are in many cases perfectly natural processes of repair, and those which nature adopts to get rid of what she cannot absorb. Not unfrequently, also, the antiseptic itself, from its possessing irritating properties, may be the cause of slight overaction. But inflammation may be moderate or excessive; pus may be laudable or putrid, and while germs may in no way affect the production of healthy inflammation, they may—as the germ theorists would assert—be the immediate cause of an unduly excessive inflammation or the discharge of putrid pus. Again, the fact of one wound presenting the appearance of undue overaction, while another, apparently under the same circumstances, shows no such signs of undue overaction, is no proof that the Germ Theory is untenable. Germs need a suitable nidus, and it is perfectly possible to conceive numerous conditions, such as those constitutionally affecting the patient or locally dependent upon the production of the injury which may, in some cases, conduce to excessive action, whilst in others to no such abnormal increase. If Mr. Bryant wishes to disprove the theory by facts, let him bring such forward as will show that under the strictest influence of those means that have been found to prevent the fermentation or putrefaction of organic fluids by the destruction of the germs upon which these processes depend, wounds will themselves become septic. Mr. Bryant professes to be an "antiseptic" himself, and uses as his "cleansing" agent a solution of iodine (20 drops to the ounce) which he maintains has, besides being an antiseptic, the additional advantage of arresting all capillary hæmorrhage. His opinion of the unhealthy action of a wound may be expressed in his own words—"I am disposed to think that it is not the mere exposure of a wounded part to the influence of air that does the harm, but its prolonged exposure." No attempt is made to ascribe any reason for this, but the germ theorists no doubt can readily enough assign one. We have attacked what we think the weak side of Mr. Bryant's article; on the other hand, did space permit, there is much worthy of notice, but we must leave it to reward those who will take the trouble to read what is a valuable contribution to the work.

The Antiseptic Method of Treating Wounds, by W. Watson Cheyne, M.B., F.R.C.S., Assistant Surgeon to King's College Hospital, and Demonstrator of Surgery in King's College, London. As Mr. Cheyne hails from the same hospital as the propounder of the great theory which his article illustrates, and as a known ardent disciple of the same, we may take it for granted that his contribution is but an epitome of the teaching and practice of Mr. Lister himself, and this fact alone will cause no little interest to be attached to it. One perhaps, of the most laudable points in Mr. Cheyne's article, is the clear distinction which he shows to exist between what is more generally known as "Antisepticism" and "Asepticism" or "Listerism." While the former is a system claimed and practised by every surgeon, the latter is a principle based upon a definite theory; and carried out with an exclusive regard for that theory. Thus, the "Aseptic" or "Listerite" believes in the existence of certain particles in the air (germs) which have the power to excite in wounds a fermentative or putrefying process, and to prevent the influence of these germs, certain known re-agents destructive to them are used. These are various, and used in various ways. So that, as Mr. Cheyne points out, it is not upon any particular parasiticide used, or any particular method employed to use it, that Listerism depends, but solely upon the principle of the Germ Theory. The spray, with which some surgeons seem to delight as much in deluging their patients, as in sopping themselves and their assistants, is no essential in the aseptic method. "The spray is a convenience (?) not a necessity." On "dressings" a definite rule appears to be followed, and as we may assume it to be the practice of *le grand chef*, it is well to notice it as differing very much from the frequent practice of many of Mr. Lister's most ardent disciples. "The dressing is always changed on the day following the operation, and afterwards the rule is, that if at the hour of the ordinary visit discharge is found at the edge of the dressing, it is changed; if not, it is left till the next day, when the same rule is followed. The dressing is never left longer than a week unchanged."

Poisoned Wounds, by John H. Packard, M.D., Surgeon to the Episcopal Hospital, and to St. Joseph's Hospital, Philadelphia. Though fitting that to the subject of Hydrophobia. Malignant Pustule, and Glanders, a separate and distinct consideration should be ascribed, yet it is a mistake that they should have been dissociated from the present article, under which they could and would have been most suitably discussed. They are briefly referred to by Dr. Packard, in contrast to other affections described, and for the better means of comparison, a complete description here would have been certainly preferable.

Sabre and Bayonet Wounds; Arrow Wounds, by J. H. Bill, M.D., Surgeon and Brevet Lieutenant-Colonel United States Army. The greater part of the article is devoted to the last class of injuries, and in a manner which suggests a perfect knowledge of this particular kind of wound. The experience of the author leads him to the conclusion that under all conditions, the arrow head must be sought for and removed.

Gunshot Wounds, by P. S. Conner, M.D., Professor of Anatomy and Clinical Surgery in the Medical College of Ohio, Cincinnati; Professor of Surgery in Dartmouth Medical College, &c. This appears to be a tolerably complete article upon the subject. The earlier portion is devoted to the consideration of wounds generally, the later to the injuries of special parts. The treatment recommended is, in most cases, ably supported by the results obtained from carefully tabulated statistics, collected from the various military reports.

The Effects of Heat, by T. G. Morton, M.D., Surgeon to the Pennsylvania Hospital, and to the Orthopædic Hospital; Consulting Surgeon to the Jewish Hospital, Philadelphia, &c. In this article are contained, besides the ordinary effects of burns and scalds, those resulting from Sunburn, Corrosive Acid, Caustic Alkalies, &c., and Lightning Stroke. The statistics with reference to the last form of injury are interesting, from showing the high annual rate of mortality it causes; thus, in France, "10,000 persons are reported as having been struck during a period of 29 years, and of this number 2,252 were instantly killed; while in the United States, during the year 1870 alone, 202 deaths from lightning stroke were recorded." Several interesting temperature charts are introduced, showing that, as the temperature rises after a burn, so does albumen appear, and its quantity proportionately increase, while the specific gravity, on the other hand, diminishes. As the fatal issue approaches, renal epithelium casts and blood corpuscles frequently appear in the urine. On the subject of treatment more might have been said upon the various methods of dealing with cicatricial deformities.

The Effects of Cold, by J. A. Grant, M.D., M.R.C.P., London, F.R.C.S., Edinburgh, Physician to the General Protestant

Hospital, Ottawa. This article deals with Chilblains, Frostbite, and the Constitutional Effects of Cold. It is very short and seems as much contributed to by the Editor as by the Author.

Abscesses, by Howard Marsh, F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital, London. Together with the discussion of Sinuses and Fistulæ, the article forms a very practical contribution.

Ulcers, by John T. Hodgen, M.D., LL.D., Professor of Surgical Anatomy in St. Louis Medical College, St. Louis. It is, we think, to be regretted that this article does not include every form of ulcer, restricted as it is to but about half-a-dozen, depending upon non-specific causes. Thus, ulcers resulting from Malignant Disease, from the different diathesis as Struma, Syphilis, Gout, &c., which are omitted, are often themselves the local diagnostic indication of the constitutional disorder, and not unfrequently the sole perceptible manifestation of the otherwise obscured diathesis.

Gangrene and Gangrenous Diseases, by E. M. Moore, M.D., Professor of the Principles and Practice of Surgery in the University of Buffalo. Under this head are included, besides the more marked forms of Gangrene, Noma, Bedsores, Boils, and Carbuncles. The portion of the article dealing with the last class is especially worthy of notice, and more particularly that part of it which describes its pathology and treatment.

Gonorrhœa, by J. William White, M.D., Demonstrator of Surgery and Lecturer on Venereal Diseases in the University of Pennsylvania; Surgeon to the Philadelphia Hospital, Philadelphia. This is an admirable article in every sense, alike as worthy of commendation for the style in which it is written as for the material which it contains. It is exhaustive on the literature of the subject, and eminently illustrative of the practical skill as well as the theoretical knowledge of the author.

The Simple Venereal Ulcer or Chancroid, by F. R. Sturgis, M.D., Professor of Venereal Diseases in the University of the City of New York; Visiting Surgeon to the Third Venereal Division of Charity Hospital, Blackwell's Island, &c., New York. In a clear disquisition on the subject, the author first discusses the nomenclature of the ulcer, showing the ambiguity with which the current terms "Chancroid" and "Chancre," are invested, and preferring in their place what rather approaches a definition than a name, "The Simple Venereal Ulcer" and "The Initial Lesion of Syphilis." The author ranks with the Dualists in his views, treating the ulcer as essentially local, perfectly distinct from syphilis, and never followed by any special constitutional symptoms. His position is well supported by his own reasoning, and the facts obtained both from individual cases and copious statistics. The article as a whole is an able and powerful exposition against the growing tendency in this country of treating chancroid rather as a mild form of syphilis than a simple and distinct venereal affection; in other words against Unicism.

Syphilis, by Arthur Van Harlingen, M.D., Chief of the Skin Clinique, Hospital of the University of Pennsylvania; Consulting Physician to the Dispensary for Skin Diseases, Philadelphia. This article is rather an elaborate compilation upon the subject than the exhibition of any original views on the part of the author. More, perhaps, than any writer, either in this or the first volume, has the author recognised the importance of giving a complete reference to any case quoted or opinion expressed. So also, where from want of space it has been impossible to adequately discuss any particular point, ample reference is given for the benefit of those who may wish further to investigate the subject. We cannot commend too highly this feature in the article, and only regret it has not been more fully recognised by other contributors. Dr. Van Harlingen may be congratulated upon producing one of the best, if not the best paper which this volume contains.

Bubon D'Emblee, Venereal Warts or Vegetations, Pseudo-Venereal Affections, Veneral Diseases in the Lower Animals, by H. R. Wharton, Ph.D., Demonstrator of Clinical Surgery in the University of Pennsylvania; Surgeon to the Children's Hospital, Philadelphia. In a few pages are discussed those diseases, which in some cases are doubtful as to their venereal origin, and others at one time considered unique but now deemed purely syphilitic.

Surgical Diseases of the Skin and its Appendages. By James C. White, M.D., Professor of Dermatology in Harward University. This is a good and serviceable article so far as it goes, but to the dermatologist there is a want of completeness from the absence of subjects which should be included, and an occasional lack of detail in those that are given. Thus the pathology of Molluscum Contagiosum is meagrely discussed; the etiology of Sebaceous Cysts but briefly referred to; Alopecia Areata is omitted, as also discoloration of the skin, as Ephelis, &c., tumours other than Carcinomata, Gangrene, Verruca Necrogenica, Ichthyosis.

Diseases of the Cellular Tissue, by Joseph W. Howe, M.D., Clinical Professor of Surgery in Bellevue Hospital Medical College, New York. The author has made the most of a subject out of which there was not much to make, seeing that Phlegmonous Erysipelas, Carbuncle, and tumours of the Cellular Tissue have been omitted as being elsewhere discussed.

Injuries and Diseases of Bursæ. By Charles B. Nancrede, M.D., Surgeon to the Episcopal Hospital, and to St. Christopher's Hospital for Children, Philadelphia. Considered either from an anatomical or a surgical point of view, this article is a really good one, and not a little of its value appears to be due to the extracts from papers by M'Ewen and Foucher, for the aid of which the author courteously expresses his "great indebtedness."

In comparing the two volumes which as yet have been laid before the profession, each contains what forbids anything beyond an equality of comparison. In the present volume the admirable articles upon venereal diseases, which take up nearly half the book, equal, if they do not even surpass in general merit, the articles upon inflammation and amputation in the first. Thus, then, while we cordially appreciated and gladly recommended the first volume, we feel, with pleasure, justified in commending to the profession the second volume under a similar sense of its value.

The Harveian Oration, 1880. By John W. Ogle, M.A., M.D., Oxon. London: 1881.

In these degenerate days, when many works (medical and otherwise) seem to be written more with a view to the aggrandisement of the author than from simple devotion to science and letters, it is refreshing to fall in with a book such as this, one which is evidently the fruit of a genuine love of learning for its own sake.

The story of Harvey's life and achievements, engrossing and all important though it is, has become almost threadbare through frequent telling in connection with Harveian "Orations," tercentenary celebrations, vivisection arguments, and disputes between the disciples of Harvey on the one hand and the adherents of Cesalpino on the other, as to priority of discovery of the circulation of the blood. It is more than an empty compliment, therefore, to say that the old tale is here re-told with such grace and point as sustain the interest unflagging to the end. The discussion of Harvey's career, however, forms but a small part of the material brought together in this volume; we venture to think that most readers, especially those who have a taste for exploring the dusty records of the past, will most relish the numerous notes, and the "adversaria, as an appendix," which form quite two-thirds of its bulk. The whole work betrays the possession by its author of a vast amount of curious and antiquarian lore, and a profound respect, as laudable as it is rare, for our forefathers in the profession, on whose foundations, laboriously and faithfully laid, we now with confidence build.

Dr. Ogle, in this spirit of reverence for the ancients, begins his oration with a short history of medicine, "especially as regards such particulars as bear on the works and the method of Harvey," this being followed by a summary of the most salient anatomical facts established respecting the circulation prior to the time of Harvey. These are:—

"1st. The distinction between arteries and veins as demonstrated by Galen.

"2nd. The non-communication between the two ventricles of the heart in man as demonstrated by Vesalius.

"3rd. The true nature of the valves of the veins as arrived at by Sylvius, St. Estienne, Fabricius, and others.

"4th. The pulmonary or smaller circulation as determined by Servetus."

Then came "the immortal Harvey, *decus et desiderium nostrum*," whose precise demonstration of the general circulation was described by the late Sir James Alderson, in 1867, as "the most prolific truth ever disclosed by inquirer into the mechanism of the human frame." The consideration of Harvey's teaching, his methods, and their results, leads naturally to a discussion of the subject of vivisection, wherein the old familiar facts and arguments are marshalled anew and directed with a weight of reasoning which is convincing enough to us, and which ought to, but probably will not (such is the protecting power of ignorance and fanaticism), throw into confusion the "bestiarian" host. After vindicating the claim of Oxford to be regarded as a really valuable and important medical school, the author alludes to the general spirit and tone of Harvey's writings, and in this connection reference is made to Harvey's firm belief in, and devout recognition of, "the hand of the Divine Architect in the structure of the outer world of nature;" passages indicating this are abundant in his works, and those, together with the terms of his last will and testament, which are most religious in character, form the only hints as to his theological views. They may be commended to the careful consideration of those opponents of science (chiefly anti-vivisectionists) who seem to think "vivisection" and practical piety incompatible.

Very few new particulars of Harvey's life are here brought to our notice. One of these is amusing:—The Earl Marshal (Arundel and Surrey) speaking of a visit to the Jesuit College at Cologne, says, "They used me with all civility," and goes on to observe, "I found in the college little Dr. Harvey, who means to convert them." In another of these letters (in the Clarendon State Papers) he is spoken of as "honest little Harvey." Another reference to Harvey, not hitherto mentioned in any systematic notice of him, is here recorded, and forms the subject of the photographic frontispiece to this volume.

"It is well known that at the battle of Edgehill, October 23, 1642, Harvey withdrew, as Aubrey tells us, along with the Prince and Duke of York, who had been no doubt for a time commended to his care, under a hedge, and took out of his pocket a book and read. But he had not read very long ere a bullet from a great gun fell and grazed the ground near him, which made him resume his station. Aubrey says nothing more of this passage in Harvey's life, but I find it related in Ant. A. Wood's *Fasti Oxonienses* (vol. iv, p. 46), that Adrian Scrope, Esq., a most valiant person, did most loyally attend his Majesty at the fight of Edgehill, where, receiving several wounds, he was stripped and left among the dead as a dead person there, but brought off by his son and recovered by immortal Dr. Will. Harvey, who was there, but withdrew under a hedge with the Prince and Duke while the battle was at its highest."

The Appendix is just such an *omnium gatherum* as the medical antiquary will revel in. The cultus of Æsculapius, the history of transfusion of blood, the credulity and incredulity of learned men, vivisection, the "induction" method, final causes, the popular estimate of the medical faculty, narcotics used by the ancients, galvanism, blood-drinking, are some of the subjects. There are many quotations we might give from these did space permit; from one of these notes, on "our former 'therapeutique way'" we cannot forbear making an extract. It refers to the illness and death of Charles II, and is copied from a MS. in the library of the Society of Antiquaries; and regarding it we would simply remark that if the moderns are wanting in classical culture and simple reverence, we know a better "therapeutic" way than that here detailed. Dr. Ogle is inclined to the belief that the king's disease was epilepsy or epileptoid convulsions, rather than the apoplexy spoken of by Halford, Macaulay, and others.

"Whilst gently walking in his bedroom, Feb. 2, 1684, he felt a certain peculiar failing of brain, which was succeeded by aphonia and severe convulsions. About 16 oz. of blood was taken from the right arm by two of the royal physicians who happened to be present. The other physicians were quickly called, and cupping to the shoulders and abstraction of 8 oz. of blood were resorted to, and an emetic of 'Infusio Croci Metallorum'* and white salt of vitriol dissolved in compound 'pæony water' was given. This was followed by some 'Pilularum ex duobus,'† and an enema, containing 'Spec. Hier. Picr.,' Syr. è Sp. Cerv. Salis Gem.‡ and Infus. Croci metal.

"Again, 'Pil. ex. duobus' dissolved in pæony water was given from time to time.

"The whole head was shaved, and epispactics applied to it ('ut nullum lapidem immotum relinquerat').

"Then tincture made of Hier. Picr. in pæony and bryony water.

"Then sneezing hellebore powder was used.

"Then (cerebri invigorandi gratiâ) a powder *è flor* Primul. Veris was used.

"Then manna dissolved in barley water, with cream of tartar (Crem. Tartar), in thin broth.

"Then, to meet strangury from the blister, an emulsion of barley water and liquorice, with peeled almonds and sugar.

"Feb. 3.—Then Spir. Salis Armoniaci was ordered to be applied to the nostrils, and the same given in a draught of *Aqua Lactis Alexiteriæ*.§

"Cephalic plasters of euphorbium and Burgundy pitch to the soles.

"There was strangury, which required an emulsion of the following—viz., dried Rad. Alth. in barley water with dried almonds and Sem. Melon.

"Throat-pain, met by gargle of Dec. of elm bark in barley water and Syr. de Alth.

"The Tinctur. Sacr.‖ and the manna with cream of tartar in thin broth were used from time to time, *and the jugular vein was opened.*

---

* The "Crocus Metallorum" was made of Sulph. Antimony and Nitr. of Potash.

† The "Pil. è Duobus" was made of Colocynth and Scammony and oil of cloves.

‡ The "Sal. Gemmæ" in the *Lond. Pharmacop.* of 1618 are described as fossilis, crystallinus, Indus, Ammoniacus, Armeniacus, niger, communis, factitius petræ, marinus, alkali, &c. *Pareira* (see 3rd ed., p. 547) speaks of the rock salt of Cheshire as sal fossilis, or sal gemmæ.

§ The "Aqua Lactis Alexiteriæ," according to the *L. Pharm.* of 1721, is composed of elm leaves, the carduus Benedictus, galega, mint, rue, absinthe, angelica, and milk.

‖ The "Tinct. Sacra." was a vinous solution of aloës.

"A Julepium was ordered, as follows:

"Aq. Ceras. nigror. Flor. Til.

"Lil. convol. Pœon. comp.

"Spir. Lavend. comp. Margaritor præp.

"Sacch. cand. alb. (ad gratiam).

"Feb. 4.—An Apozema laxativum was ordered.

"Tartar alb., Ven. alb., Fol. Senn., Man. opt., Flor. Chamæmel, rad. Gentian, Nuc Moschat, to make a decoction with water, &c.

"At night meat broth, emulsion, liquor Poseti, Cerevisia non lupulata.

"Feb. 5.—The physicians who stayed in the king's chamber at night, learning that every night there was an exacerbation or paroxysm, and some having affirmed that in the city much intermittent fever, beginning with dire convulsions, existed, giving way to Peruvian bark, it was determined to give this remedy to the king, mixed with syrup of cloves and 'Aq. Lactis Alexiter,' alternately with Spiritus Cranii humani.

"Feb. 6.—The disease getting worse, and as 'totus Medicorum Chorus ab omni spe destitutus animumque desponderit,' the king had given to him more generous cardiacs—viz., the '*Antidotum Raleighanæ major*' and Lapis Goæ* in broth; subsequently the Sp. *Salis Armoniaci Succinati*, and the Lapis Bezoard oriental.

"The brothers of the king and his heir James attended him in his last moments most assiduously.

"The king was seized with a fatal orthopnœa, and died in the afternoon of the 6th, aged 54."

We have indicated sufficiently the nature and scope of this "Oration," and would simply commend its perusal to those who not only live in the present but also love to linger on the past.

Antiseptic Surgery: its Principles, Practice, History, and Results. By W. WATSON CHEYNE, M.B., F.R.C.S., Assistant Surgeon King's College Hospital. London: Smith, Elder, & Co. 1882.

WE have at last got a work worthy of antiseptic surgery! Mr. Cheyne treats his subject in a scientific light, and therefore bestows a great deal of space to the discussion of fermentation and its causes, the life history of germs, heterogenesis, and abiogenesis. All that is necessary for the clear understanding of the various phases through which these subjects have passed, will be found in a convenient form in this portion of the work; and the matter is so arranged as to sustain the reader's attention. One cannot help admiring the ingenuity and beauty of the many experiments proposed and carried out by such an array of observers, represented on the one hand by Pouchet and on the other by Pasteur. We must not forget in these days, when the facts which Pasteur has brought forward are so overwhelming, that we owe a debt of gratitude to those who fought so persistently on the other side, for what appeared to them to be the cause of truth. Even were it for nothing more than for ably sustaining the contest; raising up barriers, difficulties, and objections, which required to be overcome and removed by Pasteur and his followers, contributing to clearness of vision and the augmentation and development of force. Hence Pouchet's strongholds have been attacked one by one, and have crumbled before the improved artillery of science. But as in war, a brave enemy is respected, so here, we admire and respect those who have honestly upheld and ably endeavoured to protect their positions.

---

* The "Lapis Goæ" was made of topaz, hyacinth, sapphire, ruby, pearls, emirals, bezoar, coral, musk, ambergrise, and gold made into a ball and polished. It was supposed to be an antidote to poisons, plague, &c., and useful 'to revive the spirits, cheer the heart, fortify nature, resist melancholy,' and 'to cause a lively presence, nimble wit, a pleasant countenance, and a sweet breath.'

Though great progress has been made, we are not at the end yet. Not long since it had been supposed that no germs could generate within the animal body, even in inflammatory products, which had not come into direct contact with atmospheric air. Now germs are found in abundance at the moment of opening acute abscesses. These, however, appear to be very innocent. But the question arises: if these germs do exist in inflammatory nests prior to their direct exposure to the air, is it not possible that other more serious germs may arise in such nests under different constitutional conditions? Again, may not these apparently innocent micrococci develop into the deadly bacteria within the body? Billroth's idea of the *cocco-bacteria-septica*, would lead directly to the belief that these micrococci are merely the bacteria in an early stage, and if this be so, then external antiseptic agents are of comparatively little value, for bacteria would generate within the body and aseptic surgery would be impossible. It becomes, therefore, necessary to find out whether these micrococci are capable of developing into some other form, and especially into bacteria. The evidence Mr. Cheyne collects, shows that the micrococci are distinct from bacteria. This is proved by chemical evidence of a varied kind, differences in their habits, and lastly, from the negative evidence of never having seen a micrococcus developing into a bacterium. These all tend to demonstrate that there is more than one species in the group of schizomycetes.

At the same time it is evident that germs which enter the system may set up serious consequences in some part of the organism, the vitality of which is lowered. The experiments of Chauveau seem clearly to prove this. If "bistournage" be performed, the testicle atrophies and disappears. There are no symptoms of putrefaction, and no bacteria appear in the part which has thus lost its vitality. If the immunity from decomposition depends in this case on the absence of putrefactive organisms, which are prevented from being carried to it from the air, then putrefaction would occur in the part, if germs were carried to it through the medium of the blood. Chauveau injected organisms into the blood of some animals, and after the fever consequent on the injection of these organisms had subsided, "bistournage" was performed, and putrefaction of the testicle occurred. In two rams injected with the same fluid and the same dose, putrefaction only occurred in the case where "bistournage" had been practised. Chauveau went still further and practised "bistournage" on the left testicle before injection, and on the right after injection, and found that the right was the only one which putrefied, proving that it is the penetration of the germs into the organs which causes the putrefaction.

Jules Lemaire gets due acknowledgment as the proposer of the germ theory of putrefaction, and also as the introducer of carbolic acid as a germicide. Mr. Cheyne says:—"In the first place we find that Lemaire recognised the true basis of antiseptic surgery, the germ theory of fermentations, and also that he was the first to use carbolic acid in the treatment of wounds." This admission places Lemaire in the very first rank of scientific surgery. "But," Mr. Cheyne says, "we do not find any method of how best to add the antiseptics to the discharge, or how best to exclude organisms altogether." This latter part was left to Mr. Lister, who had been working independently of Lemaire, and who published somewhat later the theory and practice of antiseptics, reduced to a system. Lemaire had had no hospital appointment, and no opportunity of carrying out on a large scale experiments on patients regarding the mode of dressing, and therefore we cannot say what "might have been," as Mr. Cheyne puts it, "but have only to do with what was and is." Lemaire was the promulgator of antiseptic surgery; Lister advanced the same theory and demonstrated the practicability of antiseptic surgery.

There is a considerable portion of Mr. Cheyne's volume devoted to the practical side of antiseptics, and statistics are gathered from many sources for and against their employment in surgery. The details of aseptic surgery are faithfully given, and the reader finds here clear and reliable information.

The whole volume is admirably got up; the woodcuts are numerous and well done, the printing leaves nothing to be desired; the matter is so clear and so well arranged that it reflects the greatest credit on Mr. Cheyne. All surgeons practising aseptic surgery will have the volume already in their possession; but it is especially to those who do not, as yet, practise aseptic surgery, that we recommend it. They ought to read it and think well over it, and if they do so with an unbiassed mind, we doubt not that many will be converted.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR M'CALL ANDERSON'S WARDS.

ABSTRACT OF A CLINICAL LECTURE UPON A CASE OF ANEURISM OF THE ARCH OF THE AORTA, FATAL FROM HÆMORRHAGE.—James R., æt. 49, seaman, admitted 14th Sept., 1882, complaining of pain in the chest, hoarseness, and cough, with expectoration. His father is alive, but his mother died at the age of 65 from the bursting of a blood-vessel. Eighteen years ago he had a sore on the penis, which was followed by symptoms of secondary syphilis. With this exception he seems to have enjoyed good health until five months ago, when he began to suffer from severe paroxysms of pain in the left breast, which were brought on by exertion, and about six weeks thereafter, while in Calcutta, he had, as the result he thinks of exposure to cold, what he describes as a very bad sore throat; on questioning him, however, he says that he has had no soreness of the throat, hoarseness only being complained of. This symptom after a while disappeared, but returned again a couple of months ago, soon after which the paroxysms of pain in the chest became much more severe, and cough and expectoration set in, the latter being for the most part mucous in character, though occasionally tinged with blood.

Upon physical examination on 28th October, the following points were observed. As regards the general state there was nothing special to note, and the temperature had been normal throughout. In the lungs the usual signs of bronchitis were detected, along with some dulness at the bases. The expectoration was at times tinged with blood, and a few days after admission became distinctly pneumonic. The apex beat was slightly lowered, and situated a little to the left of the nipple line. At the base of the heart a distinct diastolic murmur could be heard, which was audible up and down the sternum. These signs pointed to aortic regurgitation; but was this all? There were certain other features in the case which led to the suspicion of aneurism. Dr. Anderson having explained that the symptoms of aneurism were of two kinds, direct and indirect, pointed out that in R.'s case there were no undoubted direct symptoms, except a suspicion of dulness over the manubrium sterni, and the murmur which, however, might simply be symptomatic of aortic regurgitation. He presented, however, several of the indirect symptoms of aneurism—viz.:—(1) *persistent pain* in the left side of the chest; (2) *laryngeal symptoms*—the speech was thick and hoarse, and there was progressive dyspnœa, which latterly became distinctly paroxysmal and threatened suffocation, these symptoms probably depending partly on pressure on the trachea itself, and partly on pressure on the recurrent nerve; and (3) *the pulses* at the wrist, although no difference could be detected with the finger, were shown by the sphygmograph to be of unequal strength, the left being the stronger. Having pointed out that a difference of the pulses, or even absence of the pulse on one side, was not to be considered as pathognomonic of aneurism, as it might be due to irregular distribution, &c., Dr. Anderson passed on to the consideration of the indirect symptoms which were absent in the present case—viz., engorgement of the veins of the neck, dysphagia, pressure on the sympathetic, thoracic duct, &c. With regard to the question of cause in the present case, it was important to remember the very distinct history of syphilis. The following notes from the Ward Journal show the further progress of the case.

13th November.—On the morning of the 11th, at four o'clock, one of this man's asthmatic attacks came on, and so grave were the symptoms, which at this time pointed to spasmodic or other closure of the glottis, that the resident physician (Mr. Wm. Pattullo, M.B.), thought fit to perform laryngotomy. This gave instant relief to the more urgent symptoms, and in a short time the patient fell into a sleep which lasted for some hours. The tube then became displaced, and, as breathing was performed well enough without it, it was not re-inserted. On the forenoon of the 12th inst., however, the symptoms of spasm of the glottis returned, and the tube was again inserted. It still remains *in situ*, and to-day patient has hardly had a bad symptom: he has expectorated, however, a large quantity of the bloody mucus, which has characterised his case all along.

16th November.—At 10·30 A.M. the patient had one of his attacks of dyspnœa, and just as it was passing off large quantities of blood escaped from his mouth and through the laryngeal aperture, which was kept open by the pressure of a tracheotomy tube. He at once fell into a state of collapse, and after one or two spasmodic attempts at breathing, he died without a struggle. The pulse almost immediately after the haemoptysis fell in volume and became imperceptible. All the circumstances, therefore, connected with the case pointed to the conclusion that the patient was suffering from aneurism, probably of the posterior wall of the arch of the aorta.

Post-mortem.—The body is well nourished, and there is a small aperture in the skin of the neck leading down to the larynx. Both lungs are somewhat bulky, and, especially in the posterior parts, unduly solid. Viewed externally there is a frequent deep red discoloration visible through the pleura. On section the parts of the lungs corresponding with this are found to be largely infiltrated with blood, and, especially posteriorly, almost solidified. Towards the anterior border more isolated patches of hæmorrhagic infiltration in the midst of normally pale pulmonary tissue are noted. The bronchial tubes contain a large amount of dark red frothy material.

On laying open the œsophagus a small amount of blood is found in it; but in the trachea a large quantity is discovered near its lower extremity. In the latter there is a round bulging of the anterior and left wall, having a longitudinal extension of about 1½ inches, and beginning ½ an inch above the bifurcation. On the surface of the projection the mucous membrane is very irregular, and three apertures are discovered, one large enough to admit the tip of the index finger and very ragged, situated about its middle, and two smaller ones above it. In the middle of the large aperture one of the cartilages is exposed, ruptured transversely and projecting. The bulging is found to be caused by an aneurism of the aorta, which comes off by an aperture in the posterior wall of the arch, just sufficient to admit the point of the forefinger, and situated at a level corresponding with the space between the left carotid and subclavian. The aneurism, which forms a rounded pouch about 2 inches in diameter from above downwards, projects directly backwards.

The wall of the aorta generally presents very great thickening of the internal coat, and this thickening is much more continuous than usual. The valvular structures of the heart present nothing remarkable, but the aortic orifice is dilated. The mitral orifice admits four fingers, the tricuspid five. There is also distinct hypertrophy of the left ventricle, the weight of the entire heart being 14½ ounces. With the exception of extensive degenerative changes of all the arteries there is nothing further to note. The left recurrent nerve on being roughly traced out is found to be lost on the surface of the aneurism.

FROM PROFESSOR GEORGE BUCHANAN'S WARDS.

TWO CASES OF OPERATION FOR THE RADICAL CURE OF HERNIA. [Reported by Mr. John Goff, M.B., House Surgeon.]—CASE I. C. B., æt. 30, engineer, Motherwell; date of first admission 28th October, 1882. The following note of the first residence in hospital is taken from the report in the Ward Journal made by Mr. Wm. Pattullo, M.B., formerly house surgeon with Dr. Buchanan. At 1·30 A.M. this morning the patient was admitted to Ward III, having been sent from Motherwell in a cab by Dr. Fotheringham. It would appear that for some years he had been conscious that he had a right inguinal hernia, but as it never gave him any trouble, he had not consulted a doctor about it, and never used anything to keep it up. It came down, or rather refused to go up, on the morning of the 27th, and in a short time caused some sickness and griping pain in the abdomen. He took a dose of castor oil, but, as he got no relief, but rather the reverse, he called in a medical man about 7 P.M., who failed, after twenty minutes' manipulation, to reduce the hernia, and consequently advised him to get into hospital as soon as possible.

On admission, he was very pale and haggard, and suffering from frequent attacks of vomiting (bilious) with severe griping pains all over the lower part of the abdomen, and in the inguinal region especially. His pulse was quick but fairly strong, and the hernia pretty tense. Manipulative efforts with and without the aid of chloroform, were tried, but were abortive. Professor Buchanan was sent for with the object of performing herniotomy, but when he came, it was found that the patient had passed a motion in bed during sleep, and that the hernia had reduced itself.

Several motions were passed during the day, and the patient left hospital in the afternoon in order to make arrangements for undergoing Wood's operation for the radical cure of hernia, the nature, dangers, and advantages of which had been explained to him by Dr. Buchanan in the morning. A compress and calico bandage were carefully applied so as to control the inguinal ring and canal.

6th November.—Patient was re-admitted to the hospital to-day in order to have the operation for the radical cure performed.

11th November.—Yesterday afternoon the bowels were thoroughly emptied by means of laxative medicine, and this morning in the theatre Dr. Buchanan performed a modification of Wood's operation. After the operation a subcutaneous injection of morphia was administered; during the day he received ice to suck; and in the afternoon and evening he was allowed to have a little tea. The temperature in the evening was 100°, and the pulse 96. Between 10 and 11 P.M. he vomited the tea which he had received about an hour and a half previously, but with this exception he has remained perfectly quiet since the operation, and complained but little of pain.

12th November.—During the night vomiting occurred once. During the day he has been very quiet, has taken a little beef tea, and sucked some ice. In the morning and at night he received ʒi of laudanum per rectum. Morning temperature 102° F., evening 100°. Pulse at night 80.

13th November.—Patient feels well and has had a fair night's rest. He had a little tea and toast for breakfast, and beef tea in very small quantities at intervals during the day. Towards afternoon he began to complain of "wind" causing pain and movement in the bowels, but there is no increased pain at the site of the wound. In the evening an enema of starch with ʒi laudanum ordered to relieve the pain. Morning temperature 100·2°, evening 100·2°. Pulse very good.

14th November.—About 8·30 A.M. this morning he vomited a little dark coloured sour smelling fluid, and complained of very great pain in the "bowels on account of their constant movement." Shortly after 9 A.M. Dr. Buchanan dressed the wound, which was looking healthy, and was not at all inflamed. About 11 A.M. vomiting of small quantities of dark fluid set in, and very great pain was complained of in the bowels, but none at the site of the wound. Turpentine fomentations were applied to the abdomen, and the administration of twenty min. of liq. morph. mur., with three min. dilute hydrocyanic acid, stopped the vomiting. The fomentations were continued with great relief till the evening, when an enema was administered. Morning temperature 99° F., evening 100·2° F. Pulse weaker than usual, but regular.

15th November.—Feels much better, and the pain is almost gone. Enema repeated this morning with benefit, and the turpentine stupes continued. He received a little arrowroot. In the evening the pain returned slightly, and a subcutaneous injection of morphia was administered with benefit. Morning temperature 100° F., evening 101° F. Pulse 80, of good strength.

19th November.—Improvement is still going on. The wound was dressed to-day and is looking well; the discharge is very slight. Morning temperature 98·4°, evening 100·4°.

20th November.—To-day Dr. Buchanan cut off the lower ends of the wires after having untwisted them, but, on trying to withdraw the upper (or loop) end, he found it still firmly adherent. The wound is to-day dressed morning and night as the discharge is rather more abundant.

23rd November.—This morning the rest of the wire was removed from the upper wound, and the parts dressed as usual with oiled lint. The rings and surrounding parts are found to be firmly matted together. He now takes his food well, and the use of opium was yesterday entirely discontinued.

27th November.—The wound is healing rapidly, and there is very little discharge. He feels well and enjoys his food. The pulse and temperature are normal.

12th December.—Since the last note was made, with the exception of a slight burrowing of pus which took place a few days ago, and which was relieved by the insertion of a drainage tube for a short time, this patient's progress towards recovery has been uninterrupted, and he is now particularly well.

15th December.—Wound cicatrised and hernia cured.

CASE II.—J. C., joiner, æt. 60, admitted 10th November, 1882, suffering from an oblique inguinal reducible hernia of the left side. The history of the case is as follows:—About ten months ago, as nearly as can be ascertained, he first observed a swelling in the left groin about the size of a marble. This gave him no pain, but it gradually became larger and larger, and came farther and farther down, until, as at present, it filled the whole left side of the scrotum, forming a large pear-shaped tumour. On inquiry as to cause, the only facts that can be elicited are, that about a year ago, he fell from the lower rounds of a ladder to the ground, and about three months after this, while going along the street on a frosty morning, he fell heavily on his elbow. Neither accident seems to have troubled him much at the time. The tumour has always been easily reduced, and disappears spontaneously in the recumbent posture. There is a distinct impulse on coughing. The patient is a healthy robust man, and comes of a very long lived family.

25th November.—This morning Dr. Buchanan performed the following operation for the radical cure of the hernia. The sac was first exposed, and dissected up from the structures lying behind the spermatic cord, &c. After the neck of the sac had been carefully ligatured, the rest of it was removed. The pillars of the external ring were then brought together by silver sutures, and the external wound closed. During the day the patient's general state has been most satisfactory. An enema, containing ʒi laudanum, was administered 4 hours after the operation, and in the evening he received ⅙ of a grain of morphia subcutaneously. Temperature, evening, 99·4° F.; pulse, 84.

26th November.—Everything is going on very well. Opium has been administered in same way as yesterday. Temperatures, morning, 99·4°, evening, 98·6°; pulse, 80, of good strength and regular.

27th November.—The wound was dressed antiseptically this morning; it was looking well, and the discharge was very slight. Temperatures, morning, 99°, evening, 100·4°; pulse, 84. Laudanum enema stopped to-day, but hypodermic injection at night still continued.

28th November.—General state very good. Temperature, morning, 98·6°, evening, 101·6°.

29th November.—Wound dressed, discharge very slight. Temperatures, morning, 100° F., evening, 101·8°.

4th December.—The patient continues to progress favourably. The wound, which is dressed daily, is healing well. The pulse and the temperature are now normal.

12th December.—The wound, which is nearly healed, is now being dressed with lint spread with vaseline. The patient's general state is quite satisfactory, and the pulse and temperature are normal.

15th December.—The external wound is almost closed, but yields a few drops of pus, which seems to come from the scrotal cellular tissue.

The ring and canal are quite obliterated with the firm tissue which seems to agglutinate their edges.

[For a detailed description of Dr. Geo. Buchanan's operation for the radical cure of hernia, see this *Journal* for January, 1878, page 1.]

GLASGOW ROYAL INFIRMARY.

REPORTS UNDER THE SUPERVISION OF DR. J. A. ADAMS.

FROM MR. H. E. CLARK'S WARDS.

SCIRRHOUS TUMOUR OF THE TESTICLE AND SPERMATIC CORD.—W. M., aged 52, was admitted into Ward XX, on 2nd October, 1882, with enlargement of the left testicle, extending up the cord into the inguinal canal. He had been seen by Mr. Clark about four months before, when he was advised to become an in-patient; more recently he had been under the care of Dr. J. A. Adams, at the dispensary, who had tapped a hydrocele, and evacuated about two ounces of fluid, but finding a tumour behind the hydrocele, sent him into the house for operation. Patient stated that the disease had been present about two years; at first its progress had been very slow, but latterly more active growth had taken place. At the time of admission he was suffering much pain, had an anxious expression of face, was weak and losing flesh; the tumour was nodulated and very hard to the feel, and could be traced upwards into the inguinal canal. As far as could be made out the lumbar glands were not affected. On 6th October the parts affected were excised, it being found necessary during the operation to slit up the anterior wall of the inguinal canal and forcibly to draw down the cord; in this way the limit of the tumour was reached and a chromic gut ligature applied on the healthy cord above. At the moment of section of the cord the patient's pulse suddenly flagged, his respirations became slow and feeble, and it was found necessary to stop the administration of chloroform. For two days after the operation this condition of shock continued, and it was not, indeed, till 10th October that his condition was reported as quite satisfactory. After this he made an uninterrupted recovery, and when last seen, on 5th December, was in good health and spirits; the cicatrix was firm and healthy, and there was no evidence of recurrence.

The tumour was examined microscopically by Dr. Newman, who considered it to be a typical scirrhus; as it agreed clinically with this form of cancer, and the naked eye appearance also led to the same conclusion, there can be little doubt as to the accuracy of this diagnosis.

Scirrhus of the testicle is so very rare that many pathologists and surgeons deny its existence, such being the attitude of Rindfleisch and of Walsh. Cases have, however, been described by Sir Astley Cooper, Bryant, Nepveu, Curling, and others. Cornil and Ranvier state that scirrhus occurs in the testicle, but with extreme rarity. Before operating, Mr. Clark suggested the possibility of the tumour turning out to be of this nature; but never having seen a case of the kind before, he did not make the assertion with entire confidence as to its accuracy.

MEETINGS OF SOCIETIES.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

SESSION 1882–83.

MEETING I.—TUESDAY, 10TH OCT., 1882.

PROFESSOR GEORGE BUCHANAN, M.D., IN THE CHAIR.

I. SUPPOSED HERNIA IN LOWER PART OF THORAX. The case shown by Dr. Fraser in April was reported on as follows, by Drs. Fraser, Clark, and Finlayson, May, 1882:—

W. T., æt. 52½ years, a dyer, complains of a violent spasmodic cough, and of a severe pain, particularly in his right side, when he coughs or sneezes. He is likewise very short of breath after any hurrying or exertion.

He also complains of a swelling situated immediately to the left of the xiphoid cartilage. There is no special pain in the tumour itself, but anything beyond the slightest pressure brings on the most violent fits of coughing, something like the fits of coughing induced by inhaling very irritating vapours.

It was also found that pressure over the region of the carotid artery in the lower part of neck induced similar fits of coughing, and the left side of the neck seemed specially tender in this way. The fits of coughing are induced by drinking cold water, but he says that a drink of hot tea does not cause them. In addition, it appears that the fits of coughing sometimes occur suddenly without warning or any obvious cause.

The patient's health is now evidently broken down very seriously, although he continues to work pretty regularly, except perhaps for a day or so in the week now and then. His breathlessness and general appearance are very suggestive of pulmonary emphysema; the appearance of the chest, the quality of the percussion note, and the diminished area of cardiac dulness are likewise in keeping with this view. The respiratory murmur all over both the front and back of the chest is very good, but there is a little crackling râle at the left border of the heart. No cardiac bruit was audible. After the exertion of walking up to the house for examination, the pulse was 88; the respirations were 28 per minute, and noisy; after resting, the pulse fell to 78, and the respirations to 26.

The patient's health seems to have been quite good up to the beginning of his present illness, which he dates back more than two years ago. He seems to have had no serious illness before that except fever, and a sciatica on the left side for two years, and he had never contracted syphilis.

About two years ago he was laid up with what was called "strong inflammation" in the right side of chest, associated with severe pain there. A little cough was present then, but this appeared to be of a slight character.

In February, 1880, six weeks after he was laid down, the swelling now seen was noticed. It seems to have come on somewhat suddenly, probably within two or three weeks. At first it was scarcely noticeable, it gradually increased in size till it reached its present dimensions, and certainly it has not increased any for the last two years, although it appears to vary slightly from time to time. The cause of its appearance cannot be ascertained; there was no special strain or exertion before its appearance, but he felt, a week or so before the swelling was seen, a sudden pain about the spine, with the sensation of something slipping from its place inside—he compares this to the moving of a snib—and the patient connects this with the subsequent appearance of the lump. Ever since this illness there has been breathlessness, and a cough on pressing the tumour as described; but there has been no vomiting, only a little retching when the cough is very violent, and no disorder of the bowels has been present at any time.

The tumour consists of a more prominent part, gradually lessening as it extends downwards and to the left. It is situated between the liver on the right and the heart and stomach on the left. It is in no way discoloured, and it feels tense and elastic, but not definitely fluctuant. It does not pulsate. It appears to yield a tympanitic note to light percussion, differing in its quality from the stomach note and the note over the colon; but owing to the proximity of these organs, and also of the lung, this tympanitic percussion is perhaps less definitely ascribable to the tumour itself. There is no special respiratory murmur audible in the tumour itself, and there is no sense of crackling on handling it. There is no pain on pressing the tumour, apart from the violent paroxysms of coughing, which, however, are so severe as to limit the examination. Firm pressure fails to reduce it even to the slightest extent. It undergoes no alteration in size with the respiratory movements; during coughing it *appears* a little more prominent, but this might be due to the tension of the muscle covering it. On tracing it with the finger, its upper boundary can be felt just to the left of the xiphoid cartilage at its junction with the sternum, and this cartilage almost feels as if it were divided by the tumour, so as to form a fork, the left branch being concealed by the tumour.

1. On careful examination, we concluded that if the tumour came from within at all, it must be above the level of the diaphragm.

2. We inferred that it must come from within, as the violent fits of coughing could scarcely be supposed to arise on pressing any chronic abscess, or cystic or other tumour which was entirely outside of the chest or abdomen.

3. We thought that the tumour could not be due to a hernia of the stomach or bowel, as an irreducible hernia of these organs of such a duration, without vomiting or constipation, could scarcely be conceived to be possible.

4. An empyema, localised as this must be, could scarcely have existed for two years without pointing and bursting. The same may be said of any other form of intra-thoracic abscess.

5. A tumour growing out of the chest so as to project in this way must almost necessarily have been malignant, and in any case it could scarcely have remained stationary for two years after making its way so far.

6. A hernia of the lung, of the spontaneous variety, might account for the situation of the tumour and certain of the other facts very well; but the absence of any reduction on pressure, or of any movement during respiration and coughing, and of any specially localised respiratory murmur or crackling in the tumour itself, would make this case differ from any hitherto recorded, so far at least as our search of the literature has been carried.

7. A hernia of the pericardium must be borne in mind as a possible occurrence; and on one occasion such a tumour, in much the same situation as the present, was cut down upon by a surgeon under the notion that it was a superficial cyst. Its duration and mode of origin seem to have been somewhat similar to the present case.

8. A hernia of the pleura or pericardium, or of the lung itself, which had become, as it were, strangulated, or at least irreducible, might account for the curious phenomena here presented; if we suppose some implication of the pneumogastric nerve, the development of paroxysms of coughing on drinking cold water, and on pressing on the neck, as well as on pressing on the tumour, might become intelligible.

In consulting the literature bearing on this case, the chief attention was directed to the occurrence of hernia of the lung. The admirable thesis of Desfosses (*De la Hernie du Poumon*, Paris, 1875) gives a good account of the cases available up till 1875, including summaries of those published by Morel-Lavallée in 1847. In a very good article on "Pneumocele" in *Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques*, tome 29, p. 315, M. Merlin discusses the subject at a later date (1880). Several important articles appeared in the *Medical Times and Gazette* on "Hernia of the Lung," viz., by Cockle, in vol i, for 1873, pp. 5 and 30, and by Fox, in 1876, vol. i, p. 86, and vol. ii, p. 351.

The case of hernia of the pericardium referred to was reported in *Gazette des Hôpitaux*, but the abstract consulted appeared in *Bull. Gén. de Thérapeut.*, tome 42, p. 277.

Holmes's *System of Surgery* and Erichsen's *Surgery* contain remarks of importance, bearing particularly, however, on hernia of the lung following injuries.

Dr. Fraser said, in regard to the patient, that he had failed in health somewhat since shown to the Society in April.

Dr. Finlayson said that all the reporters were of opinion that the swelling was now fluctuant.

Dr. Cameron thought that a needle might be introduced to explore the swelling.

Prof. Geo. Buchanan thought at the first, and still thought, that this was an abscess connected probably with the sternum or the mediastinum. He would make a free incision under the antiseptic spray.

DR. MIDDLETON showed a case of CONGENITAL MALFORMATION OF THE HEART. (See last number of *Journal*, p. 446.)

DR. NEWMAN showed THREE SPECIMENS OF PATENT FORAMEN OVALE and ONE OF PERFORATION OF THE SEPTUM VENTRICULORUM.—The perforation in the last mentioned specimen was situated just below the aortic valve, and was large enough to admit the little finger. The channel was about half-an-inch long and lined by a layer of thickened endocardium, which protruded on the anterior wall of the right ventricle for nearly an inch, so as to form a valve which nearly closed the right orifice of the opening when placed against it. There was no valvular disease, and, during life, no cyanosis, but an A. S. murmur was audible at the apex. In only one of the cases of patent foramen ovale was there a history of cyanosis.

Dr. Joseph Coats, referring to Dr. Middleton's case, suggested that from the position of the murmur it rather seemed to him to arise from persistence of the ductus arteriosus than mere insufficiency of the septum.

Dr. Finlayson referred to Dr. Newman's fourth case in its clinical aspects. He had observed it carefully in the wards of the Royal Infirmary, and published the details in this *Journal* in 1874. The case turned out, as shown in the specimen, to be one of imperfect closure of the interventricular septum. During life there was a loud murmur like that of an aneurism, the patient being a boy 9 years of age. The symptoms were those of anæmia, suggesting amyloid disease, and this view seemed to be confirmed by enlargement of the spleen and albuminous urine. On *post-mortem* examination there was found a small aneurismal pouch just above the aortic valve, the thrombi in which had given rise to embolism in the spleen, kidney, &c. In this case there was a murmur along the aorta with a loud thrill, and no cyanosis.

DR. JOSEPH COATS showed a specimen of DIVERTICULA OF THE URINARY BLADDER. The chief diverticulum, which was much larger than the bladder itself, was seated immediately behind the latter, with which it communicated by an aperture in its posterior wall large enough to admit the tip of the finger.

The posterior wall of the bladder and anterior wall of the cyst were closely applied. The wall of the cyst was composed of dense connective tissue and was about an eighth of an inch in thickness. The wall was considerably infiltrated with salts deposited from the urine which was stagnant in the cavity. The edges of the aperture were rounded and smooth. There were several smaller diverticula from small pouches up to one into which the probe passed about an inch. This last mentioned one had a narrow aperture. The ureters had no connection with any of the diverticula.

The internal wall of the bladder presented irregular thickening of the mucous membrane, the summits of the folds of the membrane being encrusted with salts. The muscular coat showed also evidence of hypertrophy in the presence of reticulated prominences. It is between these muscular trabeculæ that the pouches exist.

Dr. Coats expressed the opinion that the large cyst and the smaller ones were of the nature of the ordinary diverticula, due to the mucous membrane being pushed between the hypertrophied muscular trabeculæ. The urine in this case was decomposing, and this, by causing inflammation of the wall of the cyst, may have favoured its enlargement.

In addition, there were sinuses in the perineum, and there had been extravasation of urine. The cause of this was not apparent, but the urethra was torn in front of the bulb.

Dr. Cameron referred to the clinical aspects of this case. The patient was apparently well till three months before admission, when he began to experience difficulty and pain in micturition, and to pass pus and blood. After he came into hospital there were several rigors. Dr. Cameron passed his finger into the rectum, and felt a swelling which bulged into the rectum partially closing it, and was thought to be a prostatic abscess. Extravasation of urine occurred, and this also favoured the idea of a prostatic abscess. The *post-mortem* examination revealed the true nature of the swelling, and showed that the extravasation was from a rent in the urethra, although the cause of the rent was not very apparent.

Dr. Joseph Coats showed a peculiar specimen of HYDRONEPHROSIS. In the anatomical relations there were two chief peculiarities. In the first place, the hydronephrosis is not in the usual form. The cyst, which is of very large dimensions, measuring about 9 inches in its long diameter, does not, as is commonly the case, represent the distended pelvis and calices. In its external contour it looks rather as if a greatly dilated pelvis had attached to it a greatly enlarged kidney. On opening the cyst the conditions coincide with this appearance. There is a large nearly globular cyst; then in its wall there are six apertures so small as scarcely to admit the tips of the fingers, and these represent openings of calices. On passing a probe into these, it is found that they communicate with large cavities in the kidney substance, the calices having obviously dilated independently of the pelvis. It is this dilatation which has produced the enlargement of the external contour of the kidney.

The other anatomical peculiarity is, that the ureter enters the cyst by a free aperture. It passes, indeed, obliquely into the cyst, and runs along its posterior wall before entering, but its orifice is as wide as the ureter in the rest of its course, and there is no direct obstruction. At the same time, none of the contents of the cyst escaped when the ureter was cut across, or afterwards when the cyst was handled after the ureter had been severed. The contents of the cyst were apparently decomposing urine. According to the analysis of Dr. Newman it consisted of the following:—

Albumen,	1·33	
Mucus,	·92	
Fat,	·74	
Urea,	·70	
Inorg. Salts,	·31	
		4·00
Water,		96·00
		100·00

This is obviously a case in which the ureter entered the pelvis in such a way as to produce a valved aperture in certain states of the pelvis. This being so, there would be frequent accumulations and discharges of the contents of the hydro-nephrosis. It is clear that in such a case there would not be such an excessive pressure as in the more ordinary case of complete obstruction of the ureter. This may perhaps explain the peculiarity of the dilatation of the pelvis being separate from that of the calices.

Dr. Buchanan said that during life the only indication of the hydronephrosis was a swelling which was supposed to be an abscess in process of formation.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Successful Transplantation of Muscle.—Gluck (Berlin) has shown by experiments upon animals that muscular tissue can be successfully transplanted. In February last Helferich (Munich) removed a tumour from the upper part of a man's arm, and in doing so had to remove nearly the whole thickness of the biceps muscle, to the extent of 4½ inches. To remedy this defect he transplanted a slightly longer and thicker portion of the *biceps femoris* of a dog, securing it by catgut sutures at both ends. The operation was performed with strict antiseptic precautions, *minus* the spray. At the first dressing, ten days after the operation, it was found that a portion of the transplanted muscle, the full length of the original portion, but not an eighth part of its thickness, was "necrosed." After that the wound healed rapidly. The subsequent application of electricity restored the function of the arm, and it appeared to those who observed the case that the transplanted muscle contracted under its application. This case was reported at the German Surgical Congress in June last, and in the discussion which followed Lange (New York) stated that he also had transplanted a piece of dog's muscle to fill a defect caused by an operation, and that the graft had united, but showed no signs of functional activity.—*Beilage z. Central. f. Chir.* 1882. No. 29.—D. M'P.

Amyl Nitrite in Opium Poisoning.—Two cases are recorded by Dr. E. F. Turner, of recovery from opium poisoning under the use of nitrite of amyl. Both cases were apparently bad ones, one of them being a child of six months, who had got half a grain of morphia by mistake. The inhalation was given very carefully, a few inspirations of the vapour at a time. The effect was apparent from the first, in the way of improvement of the pulse and respiration. After some hours the treatment was discontinued.—*St. Louis Courier of Medicine.* October, 1882.

Diagnosis of the Cause of Sudden Unconsciousness.—Dr. R. O. Beard, in the *Chicago Med. Journal and Examiner*, June, 1882, after treating in detail the causation of sudden unconsciousness, sums up his conclusions in the accompanying table:—

THE DIFFERENTIAL DIAGNOSIS OF

	CAUSES.	PULSE.	RESPIRATION.	TEMPERATURE.	PUPILS.
1	CEREBRAL CONGESTION.	Slow and full.	Slow and laboured. *Not* stertorous.	Above normal.	Contracted wit feeble reaction
2	CEREBRAL HÆMORRHAGE.	Slow, full, often irregular.	Slow, laboured and stertorous.	1. Below normal. 2. Later regains normal. 3. Still later rises rapidly. (105·7°)	Variably uneq insensible to light
3	MENINGEAL HÆMORRHAGE.	Slow, full, and irregular.	Slow and stertorous	Variable.	Variable, insens to light.
4	CEREBRAL EMBOLISM.	Slow, weak, and small.	Slow and full.	Slightly lowered.	Normal.
5	CEREBRITIS.	Rapid and irregular.	Shallow and irregular.	Higher than normal.	Unchanged.
6	SYNCOPE.	Rapid and thready or extinct.	Feeble and quiet.	Below normal.	Dilated.
7	EPILEPSY.	Rapid, feeble and irregular.	Difficult, irregular, often gasping.	High (to 105° F.)	At first dilated, la contracted. Inse ble to light.
8	CATALEPSY.	Normal rate, but feeble.	Slow and regular.	Normal.	Dilated and v sensitive to light.
9	CEREBRAL HYSTERIA.	Small, feeble, but regular.	Rapid and quiet.	Normal.	Unchanged.
10	INSOLATION.	At first, slow and full. Later, rapid, feeble and frequent.	Rapid and slightly stertorous.	Very high. (104°-10° F.)	Contracted. sensible to light.
11	URÆMIA.	Slow, feeble and irregular.	Slow and laboured with peculiar stertor.	Progressively lowered.	Dilated and In lent.
12	ASPHYXIA.	Slow and feeble.	Slow, feeble and gasping.	Lowered.	Variable, usu contracted.
13	ALCOHOLIC INTOXICATION.	Small, feeble and frequent.	Slow and laboured. Sometimes stertor's.	Much lowered.	Contracted. Dilate under sti lation and again lapse.
14	OPIUM NARCOSIS.	Small, feeble and irregular.	At first frequent. Later superficial and slow.	Normal or slightly lowered.	Much contract Insensible to ligh
15	CEREBRAL CONCUSSION.	Slow, feeble and intermittent.	Feeble, sighing or almost extinct.	Below normal.	Unequal. React feebly.
16	CEREBRAL CONTUSION.	Slow and feeble.	Slow but quiet.	Normal or slightly elevated.	Contracted. Insensible to ligh
17	CEREBRAL COMPRESSION.	Slow, soft and irregular.	Slow, laboured and stertorous.	Variable, usually increased.	Dilated or unequ Insensible to ligh

CAUSES OF SUDDEN UNCONSCIOUSNESS.

MOTOR POWER.	INTELLECTION.	SENSATION.	SPECIAL SENSES.	COUNTENANCE.	MISCELLANEOUS.
Partial paralysis. Reflexes impaired.	Wholly or in part lost.	Impaired.	Partially Impaired.	Flushed or Cyanosed.	Venous distention of face and neck. Retinal engorgem't.
Hemiplegia. Re- ex action abolished.	Completely suspended.	Entirely lost.	Impaired.	Variable.	Sphincters relaxed or paralysed. Deglutition impaired. Ptosis frequent.
Paralysis more or essgeneral. Reflexes nimpaired.	Suspended.	Anæsthesia accompanying paralysis.	Somewhat Impaired.	Variable.	Sphincters relaxed. Vomiting usual.
Hemiplegia, usually ight-sided. Reflex ction normal.	Transiently suspended.	Partially lost.	Normal.	Pale.	
Muscular relaxa- ion. Slight convul- ions. Reflex action uspended.	Suspended.	Hyperæsthesia.	Unimpaired.	Variable.	General evidences of inflammation.
Muscles relaxed. Reflexes unimpaired.	Suspended.	Impaired.	Impaired.	Very Pallid.	Cardiac and venous murmurs.
Convulsions. Reflex action some- vhat impaired.	Suspended.	Lost.	Unimpaired.	Pale. Ashen hue.	Eyelids tremulous Eyeballs prominent. Tongue bitten.
Muscular rigidity. Reflexes disturbed.	Suspended.	Impaired.	Partially Impaired.	Pallid.	Retinal anæmia. Eyelids tremulous.
Normal.	Partially suspended.	Blunted.	Somewhat Impaired.	Natural.	Spasmodic movements of eyes and eyelids.
Muscular relaxa- ion. Reflex action eeble.	Completely suspended.	Impaired.	Impaired.	Variable.	Skin harsh and hot. Vomiting and purging.
Epileptiform con- vulsions. Reflex ac- ion feeble.	Suspended.	Impaired.	Impaired.	Pallid.	Anasarca. Urinous odour of breath. Albumen and casts in urine.
Convulsions. Re- lex action impaired.	Suspended.	Impaired.	Disturbed.	Turgid.	
Muscular relaxa- ion. Reflex move- nents suspended.	Suspended.	Complete Anæsthesia.	Impaired.	Variable.	Odour of alcohol on the breath.
Complete muscular elaxation. Reflexes bolished. Conv'ls'ns n morphia poison'g.	Suspended.	Impaired.	Impaired.	Pale and shrunken. Sometimes livid.	Odour of opium on breath. Vomiting. Secretions arrested, except that of skin.
Muscles weak and laccid. Reflexes im- paired.	Suspended.	Entirely lost.	Act feebly.	Very pallid.	Surface cold. Vomiting frequent. Paralysis of bladder and sphincters.
Muscular rigidity. Slight facial spasms. Paralysis of eyelids. Reflexes impaired.	Suspended.	Impaired.	Impaired.	Pale.	Sphincters relaxed.
Hemiplegia. Reflexes abolished.	Suspended.	Entirely lost.	In abeyance.	Very pale.	Deglutition impossible. Sphincters paralysed. Retention of urine.

—*The Practitioner.* October, 1882.

On the Nature of what is called Sciatica.—The following is the gist of a clinical lecture delivered by Mr. Jonathan Hutchinson on the above subject:—"I believe that in nineteen cases out of twenty in which the diagnosis of 'sciatica' is suggested, there is no affection of the sciatic nerve whatever. They are simply cases of arthritic disease of the hip in one or other of its various forms—acute gout, chronic gout, rheumatic gout, subacute rheumatism, or chronic senile rheumatism. Both by the public and by the profession these cases are constantly called 'sciatica.' Our workhouse infirmaries are full of chronic cases under that name, and I speak advisedly when I say that I feel sure that they are almost all examples of *morbus coxæ senilis.* Of the cases of 'sciatica' which are not hip-joint rheumatism, some are probably affections of the fascia or of the periosteum near the hip; a minority are possibly affections of the sciatic nerve itself. In these latter it is the sheath of the nerve that becomes painful. The pain may be darting or may radiate, but it does not pass down the nerve-tubules or in any way make the patient conscious of their course. The diagnosis of true sciatica is to be based upon the discovery of tenderness restricted to the trunk of the nerve, and involving a considerable part of its course. Examples of this are decidedly rare, and their recognition without risk of error is a matter of great difficulty."—*Med. Times and Gazette.* 28th January, 1882.—G. S. M.

Rectal Examination in Hip Joint Disease.—M. Cazin, in the *Rev. de Chir.* for 1882, advocates rectal examination as an aid to diagnosis in hip joint disease. He first points out the difficulty of ascertaining the exact seat of the disease, and of determining how far the acetabulum is involved. The coxo-femoral articulation, on account of its depth, is little accessible to examination, and from this fact rectal exploration is the readiest way to approach it. On the internal surface of the pelvis, immediately above and a little behind the obturator foramen, a smooth osseous surface is met with, almost quadrilateral in shape. This surface corresponds to the border of the acetabulum, and may be designated the post-acetabular surface. The inner portion of the cavity in an adult is very thin and sometimes transparent. In the child below fourteen years of age it is occupied by cartilage.

The symptoms found on rectal touch are localised pain, revealed by pressure on the post-cotyloid surface; the presence of engorgement of the intra-pelvic glands; increase in the size of the component parts of the acetabulum; destruction and perforation of the post-cotyloid surface; swelling of the soft parts, and pelvic abscesses of various sizes.—*New York Medical Journal.* November, 1882.—J. A. A.

On the Therapeutic Value of the Seeds of Euphorbia Lathyris (L).—In a note to the *Académie des Sciences*, MM. Sudour and Caraven-Cachin record the cure of a case of poisoning by these seeds in a child, nine years of age, by astringents and opiates, and the results of some experiments they were induced to make upon themselves.

1. The principle contained in the seeds of euphorbia lathyris belongs to the class of drastic purgatives. It is unequally distributed among various seeds, some being poor in the active principle and others rich.

2. Vomiting almost always precedes the purgative action, even when the substance has been taken in a small dose. The action may be manifested in forty-five minutes; but it may be retarded for three hours.

3. The seeds produce an irritant effect on the mucous membrane of the digestive passages; their action principally bears on the large intestine, and on the pharynx in the form of pain when mastication has been prolonged.

4. In a large dose this substance produces poisonous effects, which may be divided into three periods: (1) algid (vomiting, diarrhœa); (2) period of excitement (nervous phenomena, vertigo, delirium); (3) period of reaction (heat, abundant sweat).

5. Opiates are the most prompt and effective remedies.

6. The doses prescribed in various works on botanical medicine (6 to 12 seeds) are excessive; in this dose the seeds may give rise to extremely grave gastro-intestinal irritation. Being very active, and its dose difficult to determine, this substance should not be employed in medicine.—*La France Médicale.* 18th October, 1881.—G. S. M.

Chloral in Diphtheria.—Korn in the *Deuts. Med. Wochen.*, strongly advocates the use of a solution of chloral in glycerine for cases of diphtheria, the strength varying from 15 to 30 per cent according to the age of the child. An evident caustic and antiseptic effect is produced upon the mucous membrane of the fauces. Very often the pain is relieved and the difficulty in swallowing diminished. The natural colour rapidly returns, and even the severe cases will be immensely relieved in three or four days by this simple application.—J. A. A.

The Medical Student.—This is how he is described by the editor of the *Boston Med. and Surg. Journal*, 12th October, 1882:—

"He has come to town in his usual exuberance of numbers and spirits. Somebody (we believe it is Charles Reade) makes one of his female characters speak of a young man who has disgraced himself by 'consorting with medical students and other like abominations.' It must be confessed that this lady has only given utterance to what is quite a general sentiment regarding this unfortunate being. He is not apt to be an ornamental member of society. His dissecting room experiences constitute a large portion of his conversational stock-in-trade, and he has the bad taste to introduce them on all sorts of occasions; and the chances are that he horrifies the neighbourhood by leaving a parcel of bones on the roof, to be found by an enterprising policeman, and to furnish a brief draught of scandal for the thirsty reporters.

"All this, and perhaps more, must be admitted regarding the medical student, yet, notwithstanding, he is a good fellow. He is an honest, hard worker, and he feels the satisfaction (unknown to many a less uncouth young man) of mental growth. He is no *dilettante*. He runs about all day to lectures, and hospitals, and dispensaries, and then generally gives a solid evening to his books. He gets much satisfaction out of very vile tobacco, and is usually contented with the amount of alcohol to be found in beer. Even on the few occasions when he does take recreation, he is apt to patronise that form of the drama which gives him the best opportunity to review his anatomy. He has no very high notions of art, but he does attend pretty faithfully to what he is here for. He is noisy, but good natured, and will lend a dollar (if he has it) to his companion to-day, with a cheerful alacrity second only to that with which he will borrow the same amount of somebody else to-morrow."

From all this it is evident that there is a strong family likeness between the medical student of this country and his cousin "across the pond."

THE

GLASGOW MEDICAL JOURNAL.

No. II. February, 1883.

ORIGINAL ARTICLES.

ADDRESS ON THE TREATMENT OF PULMONARY CONSUMPTION, DELIVERED AT THE GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY, 14th NOVEMBER, 1882.

By DR. M'CALL ANDERSON,
Professor of Clinical Medicine, University of Glasgow;
President of the Society.

Gentlemen,—The treatment of a disease which, year by year, carries off a considerable portion of the population of these islands, and which seems to take a special delight in seeking out for its victims those who are in the very springtime of life, and who are eagerly pressing forward towards the goal of their ambition, cannot fail to be of the deepest interest to us. And as every year brings in its train novel views with regard to its pathology and treatment, it seems well that we should from time to time review our position as to its management, and consult together as to the best methods to be pursued.

In introducing this subject we must be content with stating in a very general way the views which we are at present inclined to accept, in the hope that at some time during the session the subject may be discussed, and the opinions of the members of the Society fully elicited.

In the first place, then, we must make sure of our diagnosis: this is often an easy matter indeed, but sometimes we may, for a while at least, be in doubt whether we have before us a case of phthisis, or some other disease such as enteric fever, bronchitis, bronchiectasis, or the so-called syphilitic phthisis. It would be out of place to discuss this subject fully, but we may pause for a moment in order to say that we believe syphilitic affections of the lung to be much more common than is generally supposed, and to be usually mistaken for pulmonary consumption. And as the former affection must be treated by means of anti-syphilitic remedies, and can thus be surely arrested if seen in the early stage, we may be permitted to point out shortly a few of its more prominent diagnostic features. There is, in the first place, the history of the contraction of syphilis—generally years before in these cases which are apt to be mistaken for phthisis—and the occasional presence of concomitant syphilitic affections, such as a tubercular eruption on the skin, gummy tumours in the subcutaneous cellular tissue, deep ulceration of one tonsil. ulceration of the tongue, disease of the testicle, &c. The discovery of such conditions would lead us to suspect that the lung affection was of a syphilitic nature, but would not prove it, because there is no reason—but rather the contrary—why a syphilitic subject should not contract phthisis; but there are certain features of the pulmonary mischief itself, which, when typically present, may convert our suspicions into a certainty. In pulmonary syphilis the affection is usually unattended by fever; the right lung is much more frequently affected than the left (Pancritius found that of 94 cases, in 76 the right, and in 18 only the left was attacked); and the physical signs. instead of making their appearance at the apex, are generally at first confined to the middle portions of the chest, the reason being that the disease consists of hyperplasia of the interalveolar and peribronchial connective tissue, whose starting point is the hilus, from whence it spreads outwards and only secondarily involves the parenchyma of the lung.* Again, the examination of the sputa may sometimes help the diagnosis. For, as pointed out by Günz, little irregularly shaped, grayish-white or brownish, firm but elastic masses, which sink in water, from the size of a pin head to that of a pea, are often found in the expectoration: these present the microscopic characters of syphilitic gummy products.† On the other hand, there is a total absence of elastic fibres, and of the tubercle bacillus of Koch, such as we find in the expectoration of phthisical subjects. Finally, we should expect either recovery or decided improvement to follow upon a judicious course of anti-syphilitic treatment, which would pretty surely prove prejudicial in non-syphilitic subjects.

---

* *Ueber Lungen-Syphilis—Erfahrungen aus der Praxis.* By Dr. F. W. F. Pancritius, Berlin. Quoted from the *London Medical Record*, 13th July, 1882. P. 296.

† *Diagnose der Lungen Syphilis am Lebenden durch gummöse Sputa bei gleichzeitiger Hämoptyse.* Von Dr. J. Edmund Günz, in Dresden.—*Betz Memorabilien*, 1882. 4 Heft.

Supposing, now, that there is no doubt as to the diagnosis, our treatment must of course vary according to whether we have to deal with acute or chronic phthisis. By the former we do not mean ordinary cases of phthisis associated with fever, but those rare forms in which there is high and continuous fever, generally of the typhoid type, so that the disease bears some superficial resemblance to typhus, or to a severe attack of enteric fever, which tends to run its course in a few weeks, and to terminate fatally, unless grappled with energetically, and before the lungs are irremediably damaged. We need not stop to inquire whether the case is one of acute miliary tuberculosis, or of the so-called acute "pneumonic phthisis" (although we believe that the diagnosis can often be made), because it is now pretty generally believed that tubercle is the essential basis of both, the tubercular deposit being the prominent feature in the former, while inflammation is the most striking characteristic of the latter; but we may be allowed to express the opinion that of the two, the most surely fatal is the "pneumonic" form, because it is associated with such rapid and widespread destructive inflammation. It is unnecessary to dwell at length upon the treatment of acute phthisis, as we have already given the results of our experience in former communications, to which in the main we still adhere. The two principal indications are—first, to keep up the strength; and second, to bring down the fever. In endeavouring to fulfil the first indication the services of a thoroughly trained and reliable sick nurse are indispensable, and the hygienic and other surroundings of the patient should be satisfactory; hence it happens that when the disease is encountered amongst the working classes, who are unable to afford them, it is pretty sure to terminate fatally. The patient should be fed on fluid food every hour, or even every half hour, day and night, and stimulants are required early in the attack, and may often be taken with advantage even to the extent of half-an-ounce every hour. In fact, the dietetic treatment should correspond with that of a case of fever presenting symptoms of a similar degree of severity. The second indication is met by the use of antipyretic remedies, such as Niemeyer's antipyretic pill, consisting of a grain of quinine, half a grain of digitalis, and a quarter a grain of opium, which may be given every four hours; or large doses of quinine (from 10 to 30 grains) given once in 12 to 48 hours, and, if possible, at the time when the temperature commences to rise; or cold in some shape or other. Until recently, we have been in the habit of applying iced cloths to the abdomen in the manner formerly described, but now we prefer to use Leiter's Temperature Regulators, as they can be employed with much greater ease, and without disturbing the patient. (Instrument shown and its use explained.) In addition to these measures some assistance can be given by allowing the patient to suck ice freely, by icing the food and drinks, by sponging the body with iced vinegar and water, or even by using iced enemata. But if milder measures fail there is nothing for it but the use of cold baths, the mode of employment of which is now so well known as to require no description at our hands. Sometimes one of the above measures succeeds, sometimes another, but often a combination of them is desirable or even necessary. In addition to all this, any complications, such as diarrhœa or excessive perspiration, must of course be attended to just as we do in a case of chronic phthisis. A considerable experience points to the conclusion that if we are to bring our patients labouring under acute phthisis to the harbour of convalescence, it is by attacking them at the earliest possible moment, and somewhat on the lines indicated, but it must be done with energy, and with confidence as to the issue, else it will be imperfectly done. The members of our profession are very conservative in their instincts, and are loath to admit that a disease, almost universally regarded as necessarily fatal, can be brought within the range of therapeutics, but we believe that sooner or later it will be generally admitted that acute phthisis may be recovered from in a considerable proportion of cases, just as it is now admitted that chronic phthisis is a curable disease.

Before commencing the treatment of a case of chronic phthisis we should carefully weigh the patient, and repeat the process every week or two, a little book being kept by him for the purpose: this is of great importance as a guage of the efficiency of our measures, for a progressive gain of weight is a sure indication of improvement. In this form there is a great tendency towards a routine method of treatment such as cannot fail to be most prejudicial to the interests of our patients. On the principle that the system must be built up, beef steaks, mutton chops, cod liver oil, and strong tonics are often given in the most indiscriminate way, and without sufficient regard to the surroundings of each case, as we have seen over and over again. The time at our disposal will not allow of our considering the treatment of complicated cases, such as those in which Bright's disease, amyloid degeneration, or tubercular disease of the larynx are present; but in uncomplicated cases there are two factors which must always be specially taken into account before deciding upon a plan of treatment. The one is the presence or absence of fever, the other the state of the digestive organs: the first is more or less present in almost every case in which the pulmonary mischief is advancing, especially towards the evening, while dyspepsia is a very common concomitant, and indeed often precedes the lung disease. The fever must be attacked in the manner already specified in connection with acute phthisis, although the antipyretic remedies do not require to be pushed with such vigour, and the diet, though supporting, must be of the lightest kind, and approaching to that given to a person labouring under one of the specific fevers. The dyspepsia, on the other hand, must be treated on the same principle as we would treat similar derangements of the digestive organs occurring independently of phthisis. Into this subject it would be quite inopportune to enter fully at this time, but we may say that in addition to careful dieting, excellent results are often obtained by the use of a teaspoonful of Benger's liquor pepticus, or ♏viii to x of dilute hydrochloric acid in a glass of water an hour after meals. We cannot tell beforehand which of these will be likely to prove the more beneficial, although a deficiency of acid is more frequently at the root of the matter than a deficiency of pepsin, but each may be tried in turn; for we do not think it probable that our patients will submit to have a portion of the contents of their stomachs removed, and samples tested with these agents to see which more powerfully acts upon them as suggested by Leube. "I let the patient," says Leube, "take about twenty-five grammes of Carlsbad salt on an empty stomach, so as to cause anything which may remain in it to pass downwards; then about noon some plain, cold roast veal, with or without bread. In from one to two hours afterwards I take out a portion of the contents of the stomach with the stomach-sound, and convince myself concerning its smell and reaction, as well as how far the process of solution has advanced in the pieces of roast meat. I next put into three bottles equal quantities of the filtered contents of the stomach, and hang in each of them a bundle of boiled fibrin of about equal volume. Into one of these bottles I put nothing further, into the second two drops of hydrochloric acid, and into the third two drops of a neutral solution of pepsin. All three bottles are then placed in a large vessel of water, the temperature of which is kept at 95°-104° Fahr. The digestion which takes place in the two last bottles will show whether one or the other of the additions effects a more rapid solution of the fibrin than occurs in the first bottle, or whether they remain without import."*

We shall now suppose that fever and digestive derangement are absent, or have been removed, and that we have to deal with an uncomplicated case of chronic phthisis. Under these circumstances the remedy *par excellence* is cod liver oil, which is best given of the finest quality, and without any admixture if possible, although a little salt may be put upon the tongue before and after it is swallowed. The amount taken should not exceed 3 ounces per day, as it has been proved that when larger quantities are administered some of the oil passes through undigested; but the more nearly we can approach that amount the more likely are we to get the full benefit of the drug. In some cases Kepler's Malt Extract with cod liver oil may be substituted for the pure drug; and children often take greedily and with benefit Mackenzie's Compound Cod Liver Oil Emulsion, which, however, is generally too luscious for adults. When cod liver oil is mentioned our patients are exceedingly apt to say, "I never could take cod liver oil," or, "Give me anything but that;" and in our experience there is far too great a tendency to yield to their remonstrances, and to give instead cream or some other comparatively inefficient substitute. The fact is, that in cases such as those which we are now considering nothing can take the place of cod liver oil, and we should tell our patients that in objecting to it they are quarrelling with the best friend they have in the world. A little firmness and tact and perseverance are usually all that is required, and it is often surprising to observe in a little while how those who said they could not take it, come to swallow it in full doses and even with relish. It may, however, be omitted for a few days from time to time, if the weather is hot, or the appetite fails, or if the patient becomes bilious, in which case a little antibilious medicine may be cautiously administered, but in no case should it be discontinued for a single day after the cause for which it has been laid aside has disappeared. In many cases phosphorus may be given with advantage either in the form of pill or in the shape of Richardson's Phosphorised Cod Liver Oil; and sometimes benefit is to be derived from the use of the hypophosphites, although we confess that we are unable to endorse the extravagant encomiums which have been passed upon them in some quarters.

---

* "On the Treatment of the Diseases of the Stomach," by Prof. W. O. Leube, in Jena. *German Clinical Lectures*, p. 487. The New Sydenham Society. London, 1877.

Tonics, such as quinine, the mineral acids, and vegetable bitters, are useful adjuvants, and must be selected in accordance with the surroundings of each case. The usual accompaniment of anæmia has led to the most wholesale and indiscriminate use of iron, which seems generally to be regarded in the light of a specific in anæmic states. The view which we have been led to take with regard to the value of the preparations of iron is, we fear, very heterodox, but still we are bound to state that while we hold steel, given in the form say of Blaud's pills to the extent of six daily, to be an absolute specific in the treatment of true chlorosis, we believe it to be of very subordinate value in cases of anæmia, and we think it not unlikely that one reason why it is held in such high esteem in the treatment of the latter is, that chlorosis is often mistaken for anæmia. At all events, we have no hesitation in saying that in anæmia such as we meet with in phthisis, arsenic is infinitely preferable to iron, and it is now pretty generally admitted that iron fails while arsenic sometimes succeeds—if we see the patient early enough—in that terrible form of bloodlessness known by the name of pernicious anæmia. The virtues of arsenic have long been recognised in the treatment of skin affections (indeed in them the tendency is to give it as a matter of mere routine) and we believe the time is not far distant when it will be regarded in the light of the most valuable tonic in the pharmacopœia.

The night sweats of phthisis give rise to great discomfort and intensify the weakness of the patient, so that it is very desirable to arrest them. Being the combined result of fever and debility, they are likely to be alleviated by any of the foregoing measures which are calculated to reduce the fever and give tone to the system, and thus to some extent the treatment of this troublesome symptom is merged in the general treatment of the disease, but usually special measures are required in addition, which may not only entirely remove the sweating, but also in some cases do good in other respects. During the last two years we have, with the kind assistance of Dr. William G. Dun, been making comparative trials of various remedies, and the conclusions to which we have come may be summed up as follows:—When night sweats are present the patient should be fed by night as well as by day, and should in every case have some food and stimulant the last thing before falling asleep: he should also use a guaze flannel night dress, which absorbs the moisture without rendering the patient uncomfortably hot. Sponging the body is often of service, but vinegar and water, the usual application, while agreeable to the patient, has little effect as a rule: it is very different, however, if a tablespoonful of a mixture of equal parts of tincture of belladonna and water is employed, or a drachm of quinine dissolved in a pint of alcohol, as recommended by Dr. Currie of Lebanon.* He recommends that a small part of the body should be sponged at a time, care being taken not to expose the skin to the air. It may be repeated if required every two hours, and he holds it to be infallible. Oxide of zinc and phosphate of lime given in doses of grs. x and ℈ i respectively, and quinine and sulphuric acid in ordinary tonic doses have proved comparatively useless, while small doses (grs. iii) of Dover's powder, ergotine (grs. ii to iv) and agaricus (grs. x to xx), on the other hand, are more effectual; and we can speak most highly of the use of a saturated watery solution (about 1 in 180) of picrotoxine, the alkaloid of Cocculus Indicus. Dr. Murrell recommends ʒi of this solution to be mixed with ℥viii of water, of which he gives from ʒi to ʒiv at bed time, or ʒiii to ʒv during the day, the last dose being taken at night.† We have found, however, that this remedy is much more effectual when given subcutaneously, the initial dose being 5 min. of the undiluted saturated solution, which may be gradually increased either until the desired effect is produced, or until there is some evidence of its disagreeing. But of all remedies that which has proved most certainly efficacious is the active principle of belladonna—atropia. It may be given by the mouth, in pill or solution, at bed time, the initial dose being $\frac{1}{70}$ grain, but it is often necessary to increase the strength, which it is quite safe to do so long as the disagreeable physiological effects of the drug (dryness of the throat, dilatation of the pupils, dimness of vision, &c.) are not apparent. It is, however, preferable, when possible, to give it subcutaneously, and the solution which we are in the habit of using is gr. i of sulphate of atropia dissolved in 500 mins. of water, the initial dose being ♏v ($\frac{1}{100}$ gr.). The following case, from among many others, illustrates the value both of picrotoxine and of atropia.

Mrs. W., æt. 30, admitted to the Western Infirmary 1st September, 1880, labouring under phthisis of eighteen months' duration, involving a considerable portion of the upper part of the left, and the apex of the right lung, with moist râles. When admitted she sweated so much that in the morning her night dress and bed sheet required to be changed. On the 2nd of September the subcutaneous injection of $\frac{1}{100}$ gr. of sulphate of atropia was commenced, and continued for about fourteen days. The sweating during the day ceased at once, and at night it became gradually less, so that at the end of the fortnight it had ceased altogether. The injections were then accidentally discontinued, and slight sweating again appeared, but never to the same extent as before. On the 22nd October the hypodermic injection of a saturated watery solution of picrotoxine was ordered, the initial dose being 3 mins., to be increased by 2 mins. each night until the sweating was controlled. This was continued for four nights only when it was stopped on account of the skin at the sites of the punctures becoming inflamed. Slight perspiration was observed by the patient for two nights, after which it ceased completely. The only other medicinal treatment consisted in the use of a simple cough mixture, and up to the date of her dismissal from hospital, upon 8th November, 1880, there had been no return whatever of the sweating.

---

* *Michigan Medical News.* Quoted from the *London Medical Record,* 25th July, 1882, p. 271.

† *The Practitioner.* October, 1879.

Sometimes the best results are obtained by giving the atropia and picrotoxine solutions in combination; but whatever drug is used we must on no account stop it whenever the perspiration has been arrested, else a relapse is pretty sure speedily to occur, as in the case just quoted.

It may seem strange to some that we have not hitherto alluded to cough mixtures, which appear to be so popular with the profession in the treatment of pulmonary affections, but the reason is that we hold them to be of very subordinate importance, and that taken overhead mankind would probably be gainers and not losers were they entirely discarded, for not only are they often ineffective, but they are apt to derange the digestion and to take away the appetite. We do not, of course, mean to say that they should never be employed, but charily and with great discrimination, and when given it is well to combine them with tonics; thus, ♏iii of dilute hydrocyanic acid and ♏v of ipecacuan wine, with x of spirit of chloroform and dilute nitric acid may be given with ʒss of infusion of calumba; but often we can best relieve the cough by giving an opiate at night such as ♏xxv of chlorodyne, leaving the stomach unfettered to perform its functions by day.

It is far otherwise, however, with regard to the inhalation of medicated vapours. These have long been used to a certain extent, even in the times of Hippocrates and Galen, though often with a wavering hand, and on theoretical grounds rather than with a practical hope of success. But recently a new impulse has been given to such measures by the remarkable results of the investigations of Koch and others, which seem to show that there is a veritable "materies morbi" at the root of consumption—the "tubercle bacillus"—and which can best be destroyed by attacking it directly in its citadel. Indeed, the fear now is that antiseptic inhalations will be used in the most wholesale way, and without due regard to the surroundings of each case. For it must never be forgotten that the more the acute form of the disease is approached, the more cautious should we be in the use of stimulating vapours. In carrying out this treatment we confess that we have a preference for the naso-labial respirator of Mackenzie, or Mayer & Meltzer's modification of the same, or—in the case of hospital patients where expense is an object—Dr. Burney Yeo's cheap substitute * (instrument shown); but whatever instrument is employed it should not be used for more than one patient, even after disinfection, if it can be avoided, owing to the possibility of transmitting the disease to others. As all are aware, an immense variety of medicaments have been used, but the one for which we at present have a preference, of which we have had most experience, and from which good results have been obtained, is a mixture of equal parts of *pure* creasote and spirit of chloroform, the latter serving to dilute the former, while at the same time it acts as a sedative to the inflamed surfaces, and tends to relieve the cough. The respirator may be used at first for about ten minutes two or three times a day, but whenever the patient has become accustomed to it, it may be employed for gradually increasing periods until at last it is worn all day. It is too soon to speak very dogmatically about the value of this method of treatment, but we believe that a bright future lies before it, if it is not employed indiscriminately, but only in properly selected cases, although we fear that he would be a sanguine man who would predict that it will ever prove to be a veritable specific. For it must never be forgotten that, just as in actual war, it is often by a flanking movement rather than by direct attack that we can achieve a victory over disease.

But of all antiseptics it will be admitted that the most valuable by far is pure air, which, with suitable surroundings, is justly regarded as a most important factor in the treatment of chronic phthisis; and yet we find that a common custom prevails of cooping up such patients in a couple of rooms during the whole of the winter months, regardless of the serious disturbance of the general health which almost inevitably follows upon such a course. The question of climate, then, —especially during the winter and spring months—merits the most serious attention of those who would grapple successfully with this disease. It would be quite out of place, within the scope of a paper, to discuss this subject at length, but we may say that, without desiring to depreciate in any way the value of the health resorts in the south of England, the Riviera, &c., we have seen the best results to follow upon a residence in high mountain valleys, and from long sea voyages. As regards the former we have an abundant choice in various parts of the globe, such as the Neilgherry Hills, the Kirghiz Steppes of Asiatic Russia, Davos Platz and the Engadine in Switzerland, and numerous health resorts in the United States, Ecuador, and Mexico. We have already in a former communication* sufficiently indicated the special virtues of a residence at Davos Platz, which is peculiarly applicable to those who have sufficient means at their disposal, and whom a cold and bracing climate suits. In the opposite class of cases many of the American health resorts are to be recommended. As types of these we may select Colorado, and Quito in Ecuador. Dr. Charles Denison, who was himself cured of phthisis by a residence at Denver, a city lying at an altitude of 5,200 feet, has recently published an interesting volume on *Rocky Mountain Health Resorts.*† He tells us that from January 1874, to July 1879, *i.e.*, four and a half years, only fourteen cases of phthisis are recorded as having originated in Denver, several of which at least he found on inquiry to have been erroneously classed. For consumptives visiting Colorado he specially recommends camping out. "Let the invalid tourist," says Denison, "on his arrival in Colorado, remain ten days or a fortnight in one of the lower towns—Boulder, Denver, Pueblo, or Canon City, as the case may be—and from thence, if desirable, advance gradually, by rail, horseback, or wagon, to higher levels, as the enfeebled lungs accustom themselves to the rarefied air.

---

* Which may be obtained for a few pence from Messrs. Corbyn & Co., 300 High Holborn, London.

"One of the best methods of gaining the altitudes, and of obtaining the highest possible benefit from air, sunshine, exercise, and elevation, is by *camping out.* In the pre-railroad days, when all who crossed the plains were compelled to do so in a wagon, or with an ox-team, the degree of improvement was greater among the consumptive invalids than it is at the present time, because then all phthisical patients even though they left home upon a mattress, *must* live in the dry open air, sleep under the stars, and often *do their own cooking*. . . . .

---

* *Glasgow Medical Journal.* April, 1880.

† Boston: Houghton, Mifflin & Co. 1881.

"The charm of this unique country lies in its variety, its capability of developing new and interesting features, and the novel experiences it offers wherever one may turn. The mountains, with their beautiful parks and canons, are accessible from most of the first stopping points upon the railroad, by less than a day's ride.

"Armed, equipped, and outfitted, a party may follow one of the creeks up a rugged canon, camping at nightfall upon the banks of the stream beneath the crags; finally they reach a park above, where they pitch their tent in a wooded vale near a tumbling mountain stream, or,—

> 'By shallow rivers, to whose falls
> Melodious birds sing madrigals,'

and spend weeks delightfully, sketching, botanising, geologising, fishing or hunting, but always and ever recuperating. . . . . As one has wisely said, 'No one need be afraid of the sunlight of Colorado. It has all the beneficial effects of sunlight in other countries, with none of its enervating effects common elsewhere. *Bask in it*, enjoy it all you can, for few have as yet fully appreciated the beneficial effect of the chemical action of sunlight on the blood.'

"As for the sunsets of Colorado, they are, as we have said, truly unsurpassed. No artist, without incurring the imputation of exaggeration, could do full justice to the vivid tints and gorgeous colours which bathe our western skies after the sun has sunk below the mountain horizon.

"To see one of these camping parties coming in from the mountains after 'roughing it' for a month or two, is sufficient to convince the most incredulous of the utility of the *régime*. They left the plains thin, languid, and pallid; they return bronzed and rugged, with elastic tread and full chests, gladly owning that to the experiences of camping out they owe a new lease of life. It is an opinion which I have previously expressed, that to the fact of *sleeping upon the ground* in the pure dry air, amid the balsamic exhalations of this primitive resting place, may be attributed much of the happy result of camping out. The system, roused by the tonic influences of earth and air, wakes into new life and vitality, and morbid feelings and conditions wear away."

"Quito, in Ecuador—on the line—with a population of 70,000, is about 9,000 feet above the level of the sea, and backed both east and west by mountains, the highest of which has an elevation of nearly 20,000 feet. It is thus well sheltered, and the atmosphere is both dry and warm, and equable. Dr. Domec, who spent about four years there recently, states "that in a large room, with the door and window open day and night, he found the temperature to oscillate all the year round between 57° and 65° Fahr. . . . . and that during four years he watched daily the thermometer, placed in a large drawing-room of the house in which he lived, without a fire, and open to every wind day and night. He never saw it, between 6 P.M. and 10 o'clock at night, above 63° or below 57° Fahr. Sometimes in the night, with the wind from the mountains, the thermometer was lower, but the falling of the temperature was always of short duration, and its fall never reduced that of the room below 57°. . . . . He only saw two or three cases of spontaneous phthisis among natives during that time, and in all cases of inherited phthisis from the seacoast that he met with, the progress of the disease soon appeared to be arrested."*

As regards the latter (sea voyages) it must be noted that a voyage has a very different effect from a residence on the coast, and sailors have frequently informed us that they always suffered from colds when on shore, which never disappeared till they were fairly out at sea again. One of the most suitable voyages, and from which most excellent results have often been observed, is round the Cape to Australia in a first class ocean steamer, the voyage being so timed that the patient leaving this country in October arrives in Australia about the close of the year. But a mistake which is very commonly made by patients who have recovered their health on the voyage out, is to disregard instructions, and, instead of returning after a short stay, to remain in Australia, and especially in towns such as Melbourne, where consumption is very rife, and where all the improvement they have experienced is apt soon to be lost, too often never to be regained. Much benefit is also frequently obtained from a trip to the Mediterranean during the winter and spring months.

We are well aware that many points in connection with the treatment of phthisis have not even been touched upon, and that others have been treated in a most cursory way, but all that we could attempt to do was to give a slight sketch of the views which our present experience has led us to adopt, and we shall consider that the time of the members of the Society has not been entirely misspent if we have succeeded at any point in interesting any of you with regard to a subject of the deepest importance, and in connection with which a wide divergence of opinion may reasonably be entertained.

---

* *Consumption as a Contagious Disease.* By Daniel Henry Cullimore. London: Baillière, Tindall & Co.

KOCH'S RESEARCHES ON TUBERCULOSIS.

By GEORGE A. HERON, M.D., London.

On the 24th of March, 1882, in a paper read before the Physiological Society of Berlin, Dr. Robert Koch claimed to have established, by experiment and by observation, the existence of a micro-organism which is associated with tubercle, and not only associated with tubercle, but, according to Koch, the cause of all tubercle. This organism is a bacterium of the kind known as a bacillus, and it is, consequently, rod-shaped. In length it varies from about $\frac{1}{3000}$ to $\frac{1}{12000}$ in., and its breadth is about $\frac{1}{5}$th of its length. In looking at a specimen of these bacilli, it will be seen that certain of them contain spores, two to four, ranged along the length of the organism.

In attempting to give an outline of some of the points on which Koch lays especial emphasis in the lecture referred to above, it is obvious that attention must be given solely to what is there stated. Since that date no observations have been published tending to disprove Dr. Koch's work. On the other hand, the bacillus described by him has been found by several observers in the tissues and in the sputa of persons whose conditions of disease would have suggested to any clinician, of ordinary experience, the probability of the presence of tubercle in the patient. It must, then, be admitted, that we have now to deal with a new fact which characterises, by the presence of these organisms, certain cases of disease of well known type, about the exact clinical significance of which there is, even now, no inconsiderable difference of opinion. For those who find themselves justified in accepting Koch's results as true, all difficulties about the nature of these cases must cease, as soon as it is found that the patients concerned harbour in their tissues, or in their secretions or excretions, this bacillus of tubercle.

The bacillus is demonstrated in tissues by employing the process first described by Professor Ehrlich. Koch has adopted this process in preference to the one devised by himself, and with the aid of which he worked out all his early experiments. Ehrlich's process will be found fully described in the *British Medical Journal* of 14th October last, and Professor Vignal makes some useful remarks upon the process in the same *Journal* on 28th October. It is, for these reasons, unnecessary here to touch upon the method of investigation required for the detection of the bacillus. There is, however, one error in my remarks as they appear in the *Journal* of 14th October. I ought to have said, that the bacillus of leprosy gives precisely similar results with those shown by the bacillus of tubercle when these two organisms are submitted to the process of staining devised by Ehrlich. There are, however, some differences in form, as Dr. Koch points out, between the two bacilli. The bacillus of leprosy is "more slender and more pointed at the ends" than that of tubercle. They are also distinguished from one another by the colour test of Weigert, to which the bacilli of leprosy respond; those of tubercle, on the contrary, are unaffected by it.

Dr. Koch thus describes the appearance of the bacillus in tuberculous tissues:—"In all cases where the tuberculous process is in its early stage and progressing rapidly, the bacilli are to be seen in great numbers. They then lie thickly, and often in groups or small bundles inside the cells, and in some places give the same appearances as the bacilli of leprosy when they are found in cells. Near these (groups or bundles) are found numerous free bacilli. Especially on the borders of large cheesy deposits crowds of bacilli appear, which are not shut up in cells."

"As soon as the highest point of the tubercle eruption is overstepped the bacilli become rarer, or are only to be found in little groups or singly at the edges of the tuberculous deposits, and lying near them are bacilli so faintly coloured as scarcely to be recognisable; these are presumably already dead or in the act of dying. Finally, they can quite disappear, although they are rarely altogether absent, and then only in such places as those in which the tuberculous process has come to a standstill."

In his lecture, Dr. Koch lays emphasis upon the connection which appears to exist between the presence of the bacillus and of the giant cell. "If," he says, "in the tuberculous texture giant cells appear, then the bacilli lie by preference in these structures. In cases of very slowly progressing tuberculous processes, the inside of those giant cells is generally the only place where the bacilli are to be found."

Dr. Koch has a theory about the connection between the giant cell and the bacillus, and it is this:—"It is to be concluded from the size and position of the giant cells containing bacilli, that these cells are the youngest, while, on the other hand, those cells which are free from bacilli are the oldest, and it is to be assumed that these last originally contained bacilli, and that the organisms have either died or have gone over to that condition which will presently be described. From the observations of Weiss, Friedländer, and Laulamié, according to whom giant cells were formed around foreign bodies, such as vegetable fibres and the eggs of strongylus, we may be able by analogy to realise the relation of the giant cells to the bacilli. We may infer that here also the bacilli, as foreign bodies, are enclosed by the giant cells, and on this account, if the giant cells are found empty, all further relations of the tuberculous process go to shew that the presumption is correct, that the giant cells had formerly harboured one or more bacilli, the organisms having occasioned the origin of the cells." So much for the appearances described by Koch as illustrating the presence of the bacillus of tubercle in tissue, and its peculiarities there. It makes no difference whether the bacillus is seen in a human being or in a monkey, a guinea pig, a mouse, or a hen, the organism is always the same in every detail.

And now as to the facts upon which Koch, on 24th March last, founded his claim for the recognition of this organism as associated with tubercle. He found the bacillus present in the following cases:—

1st. In the human subject—
    11 cases of miliary tubercle.
    12 cases of cheesy bronchitis and pneumonia. (In six of these cavities had formed.)
    1 case of tumour of brain of the size of a hazel nut.
    2 cases of freshly extirpated scrofulous glands.
    2 cases of synovial degeneration of joints.
    Twenty-eight cases in all.

2nd. Amongst the lower animals—
    10 cases of perlsucht of the ordinary type.
    1 case of caseous cervical gland in a pig.
    1 case of a hen which died of tubercle.
    3 cases of spontaneous tubercle in apes.
    9 cases of spontaneous tubercle in guinea pigs
    7 cases of spontaneous tubercle in rabbits.
    Thirty-one cases in all.

"Besides these cases of spontaneous tubercle, I examined," says Koch, "172 guinea pigs, 32 rabbits, and 5 cats, all of them infected with tubercle by the inoculation of the most varied tubercular substances, such as gray and calcified tubercle of human lung, phthisical sputum, tuberculous masses from spontaneously diseased monkeys, rabbits, and guinea pigs, pieces of lung from cattle suffering from perlsucht, cheesy as well as calcified, and, lastly, by inoculation from tubercular affections produced in animals by inoculation." In each of these cases, 232 in all, bacilli were not once wanting, and in many instances they were extraordinarily numerous. So much, then, in proof of the statement that this particular bacillus is found associated with tubercle.

And now comes the second point. It remains still to indicate the line of evidence advanced by Koch in proof of the belief, that this bacillus, and nothing but this bacillus, is the cause of tubercle. To prove this, he carried out a series of experiments in which he took tuberculous particles from animals which had either died of tubercle, or, having tubercle, had been killed for experimental purposes. These particles were about the size of millet seeds, and were removed from the dead body and placed upon the blood serum, prepared in a certain way, of the ox and of the sheep, with scrupulous attention to all those precautions which are familiar, at least in theory, to every one who is acquainted with what are known as "cultivation experiments." The object of these elaborate experiments was to obtain the bacillus of tubercle free from all taint. Dr. Koch believes that he succeeded in attaining this end. After describing how he sowed the tubercular morsels upon the prepared blood serum and watched their slow growth, and noted certain of its peculiarities, he makes a statement which deserves special attention. He says, "The extremely slow growth, which alone is to be obtained at breeding temperature, and the peculiar shovel-shaped, dry, and firm condition of these colonies of bacilli, are not to be found in connection with any other known bacterium, so that, the confounding of the culture of the tubercle bacillus with that of any other bacterium is impossible; and already, with only short experience, nothing is easier than to recognise at once accidental contamination of the culture." This is a very important statement, and it is all the more important when it is made by Koch, one of our best experimenters and observers, and one whose words carry with them that authority which can be given only to a profound and extensive knowledge, such as his, of the life history of bacteria.

After describing the appearances of the growth of the bacillus under cultivation, he says, "By the help of a low power (30 to 40 diam.) the colonies of bacilli" (undergoing cultivation) "are already visible towards the end of the first week. They appear as very elegant spindle-shaped and S-shaped structures, and also in other similar crooked figures, which, if they are spread out on a cover glass, coloured" (*i. e.*, submitted to Ehrlich's colour test), "and examined with a high power, consist solely of the familiar extremely delicate bacilli." Had any other known organism been present, it is hardly within the bounds of possibility that Koch could have failed to observe it, for upon the accuracy of this observation hinges much of the worth of his researches into the nature of tubercle.

These cultivation experiments were carried on for some time. After from ten to fourteen days' cultivation in one test tube containing the prepared blood serum, some of the crop of the bacilli which had grown there was transplanted to another test tube, and after another ten days or so, some of this second crop was sown in a third test tube, and so on, until, in one mentioned instance, the cultivation extended to 178 days.

On such observations and experiments as these rests, in part, the proof that the bacillus of tubercle was obtained free from taint.

Dr. Koch next proceeded to inoculate certain animals with the pure bacillus, obtained by cultivation. The inoculation was always performed with every care against the possibility of contamination. In each series of experiments several animals were used, including rats, mice, guinea pigs, rabbits, a marmot, pigeons, frogs, &c. Some of these animals are, it is well known, difficult to infect with tubercle—a fact which is not without some significance in this connection.

Koch thus sums up the results of these inoculations:—"If one looks back upon these experiments, one sees that a not inconsiderable number of animals were experimented upon, on which bacillus culture was brought to bear in very different ways—viz., through simple inoculation into the subcutaneous cellular tissue, through injection into the abdomen or into the anterior chamber of the eye, or direct into the blood stream, without failing, even in one single instance, to develop tubercle; and there had formed in them not solitary nodules, but an extraordinary mass of tubercle corresponding with the large number of infecting germs introduced." In each of these series of experiments—there were thirteen in all—a certain number of animals were not submitted to the inoculation of the cultivated bacilli. These animals had been bought at the same time, and fed and lodged in the same way and in the same places with those animals which were inoculated with the bacilli, but not one of the former shewed any evidence of tubercle, either during life or after they had been killed and examined *post-mortem.* It must be remembered that these cultivation experiments were made with tubercle taken from the lungs, calcified mesenteric glands, and freshly extirpated scrofulous glands of human subjects, as well as from the lower animals, and that there was no difference whatever in the effects produced by inoculating from these two distinct sources; and the bacilli from these two sources were also identical in appearance.

Dr. Koch makes some very interesting observations about certain distinctions which he draws between tubercle occurring spontaneously in an animal and that type of tubercle which results from inoculation. He bought and examined one hundred guinea pigs, all of which were quite healthy. Several of them were shut up in a room with other guinea pigs which had been inoculated with the tubercle virus. In three or four months, but never before the lapse of that time, spontaneous tubercle began to show itself, and always sporadically, amongst the uninoculated guinea pigs. In them the bronchial glands were "always found unusually large and purulent, particularly, also, in the lung was to be found a large cheesy mass, with very far advanced breaking down in the centre, so that, sometimes, as in human beings, it had reached to actual cavity. The development of tubercle in the organs of the lower part of the body was very far behind that in the lung. The swelling of the bronchial glands, and the commencement of the development of tubercle in the organs of breathing, leave it beyond a doubt, that the tubercle of these animals was an inhalation tubercle springing from a few or possibly only one infectious germ, and on that account very slow in its progress."

Contrast that description with what Koch says about inoculated tubercle, and the contrast will be found to be very suggestive. "The place of inoculation was in the belly of the animal, near the inguinal glands." The first sign of the success of the inoculation was the appearance, at the end of a week, of a nodule over the site of the puncture. About the end of the second week, the inguinal glands beside the wound began to swell, and sometimes also the axillary glands. From that time the animals grew quickly thinner and died in from four to six weeks, with marked tubercular affection of the liver and spleen, those organs having been but slightly affected, as compared with the lungs, in the cases of spontaneous tubercle.

Several animals were inoculated with certain substances which did not contain the bacillus of tubercle; for example, morsels of a scrofulous gland, of degenerated synovial membrane from a joint, of a portion of monkey's lung kept dry for two months, of another portion of the same lung which had been kept in alcohol for one month, and in not one instance did the animals experimented upon with these substances shew any sign of tubercle either during life or *post-mortem.*

Several experiments with sputum from tuberculous individuals are mentioned in Koch's lecture. The sputum was allowed to dry, as it may sometimes be seen drying on a hospital floor, not always in an out of the way corner. Tubercular sputum, dried in this way, was found to be as surely fatal in its results, when an animal was inoculated with it, as had been the case when the cultivated bacillus was used. The specimens of sputa with which these experiments were made, were from two to eight weeks old.

A highly suggestive series of experiments were performed, with the view of ascertaining the effects of varying quantities of bacilli upon animals, into which the organisms were introduced by injection. The anterior chamber of the eye was selected as the site of the experiments. In one case the pure prepared blood serum, used in the cultivation of the bacilli, was injected. It was, however, in this instance, unmixed with the bacillus or any other organism. It was injected; and the animal was killed and examined thirty days after the operation. All its organs were found healthy. No bacilli of tubercle were seen although they were carefully sought for. In another case the injection was made with blood serum mixed with bacilli, which had been cultivated during 132 days. The needle of the syringe was pushed into the anterior chamber of the eye, but the piston of the instrument was not moved. In this way only an extremely small number of the bacilli could have entered the eye. In a fortnight from the day of the puncture, solitary nodules, of a light golden tint, appeared upon the iris near the site of puncture. From that time tubercular iritis was developed, and the cornea became cloudy. In thirty days the animal was killed, and, besides the changes in the eye, the glands near the jaw and at the root of the ear were swollen and contained yellowish-white deposits. In two other cases the injection was made with blood serum charged with cultivated bacilli; but many drops were introduced into the anterior chamber of the eye. These two animals also developed the local symptoms indicated in the last case, and they rapidly became thin. In thirty days they were killed, and, in addition to the local changes, their lungs contained "innumerable tubercles." The lungs of the animal subjected to the inoculation of a minute portion of the blood serum free from bacilli were free from all sign of tubercle, and so were its other organs.

Experiments exactly similar to the foregoing were repeated again and again, and invariably with like results.

Koch thus begins to sum up and give what he regards as the outcome of his work. He says—"All these facts taken together justify the conclusion that the bacilli present in tubercular substances are not merely the associates of the tubercular process, but the cause of it, and that we have before us, in bacilli, the actual tubercle virus. It is also possible, by this means, to draw the boundary of those diseases regarded as tubercular, which, hitherto, could not be done with certainty. A decided test for tubercle is wanting, and one man considers miliary tubercle, phthisis, scrofula, perlsucht, &c., to be tubercle; another man holds, perhaps with equal right, that all these processes of disease are different. In the future, it will not be difficult to decide what is tuberculous and what is not tuberculous. Not the peculiar structure of tubercle, not its non-vascularity, not the presence of giant cells will decide the question, but the presence of tubercle bacilli—be it in the tissues by the colour test, or be it through culture on prepared blood serum. This criterion, taken as a guide, must, according to my researches, stamp miliary tubercle, cheesy pneumonia, cheesy bronchitis, tubercle of glands and of the intestine, perlsucht in cattle, inoculated and spontaneous tubercle as identical."

Many points of great interest have not been touched upon in this attempt to summarise Dr. Koch's work on tubercle, which received from him, in his lecture, considerable attention. It is hoped, however, that enough has been said to indicate the lines on which this admirable piece of work has been founded and built. In thinking over it, many questions will suggest themselves to the mind, and answers will be sought for them. If these answers rest upon facts, then only good can follow when medical men come to consider the questions and their answers. If, however, mere personal opinion, founded upon merely personal bias, takes the place of facts, then we shall have discussions which will settle nothing and end nowhere. One question can hardly fail to be amongst the first to suggest itself—How does all this work of Koch's fit in with the hereditary nature of tubercular phthisis? If there has been any fact established in medicine by the evidence of patients, and the practically unanimous opinion of physicians of all countries, it is certain that the hereditary tendency of tubercle has been so established. Of late, however, in Germany, in France, and in America, as well as in our own country, some men have expressed doubts as to the soundness of the evidence upon which rests the all but universal belief in the heredity of tubercle. Of course, this is a perfectly legitimate position, and besides that, it has been taken by men, some of whom are entitled, from their clinical experience, to speak with authority upon the subject. Others have gone still further, and have denied the truth of the hereditary tendency of tubercular phthisis altogether. Koch does not fail to touch upon this point in his lecture. He takes up no definite position in connection with it, but simply indicates the opinion that we must have still further inquiry into this question.

I think that, with his views, such an opinion is what might be expected from the man who has done such work as his.

We cannot yet tell what light will be thrown upon disease by Koch's great discovery. I call it a discovery, because whatever may be our attitude with regard to the bacillus of tubercle, it is true that Koch has shown us, in the presence of the bacillus in a certain type of disease, something which we did not know before.

It is only seven months since this discovery was given to the medical world. No doubt, many men have been engaged, since last March, in studying the clinical bearing of the tubercle bacillus. To speak about establishing clinical facts concerning a subject such as this in seven months, would surely be to misuse words. I have, however, had the advantage not only of observing my own cases, but also of receiving from Dr. Bristowe a short statement of some of his observations in his wards in St. Thomas's Hospital, in this city. In a letter which I received from Dr. Bristowe, dated 22nd October last, and from which he kindly allows me to quote, he says, after detailing some cases under his care in which the bacillus had been found, "My experience, as you see, is limited; but so far as it goes it confirms the belief that a special form of bacillus exists in tubercle, and may be found in the sputa of phthisical patients. It tends, also, to show that the examination of the sputa for bacilli is an important method of determining, in doubtful cases, whether we have tubercular or some other disease to deal with. My own experience does not tell me whether the bacilli are chiefly abundant in the sputa, in cases in which the lungs are breaking down. But I may observe that all the phthisis cases examined by me were well marked cases, and probably all had excavations."

At my request Dr. Lawrence Humphry, formerly Resident Medical Officer at the City of London Hospital for Diseases of the Chest, now resident at Cambridge, made a short statement of the results of his observations in connection with the tubercle bacillus in the hospital. It is as follows:—

"I.—*Cases of advanced phthisis with high temperature, &c.* —The sputum, in all cases examined, was highly charged with bacilli.

"*Post-mortem Examination.*—The fluid from the cavities in the lungs, the scrapings of the caseous nodules from different parts of the lung, the caseous parts of the bronchial glands, and in one case the mesenteric glands, contained them in large numbers. I did not find them in the tubercular ulcers of the intestines.

"II.—*Cases of acute secondary tuberculosis.*—In these the sputum contained a larger quantity of bacilli than in any other cases, as did also the fluids from different parts of the lungs. Two or three of them were remarkable for their acute progress and rapid termination.

"III.—*Cases of incipient phthisis.*—In most bacilli were found in small numbers, one or two in a field. In some no bacilli were found after repeated examinations.

"IV.—*Bronchitis, subacute, chronic, and asthmatic.*—No bacilli in the sputum.

"V. *Chronic fibroid.*—In cases complicated with caseous pneumonia or secondary infective processes, the sputum contained bacilli; also the fluid of the lung cavities."

That is what Dr. Humphry has to say on this subject. I know that he has taken great care to make his observations exact, and I am under a debt of gratitude to him for his kindness in supplying me with material for my own work, even, as sometimes happened, at considerable inconvenience to himself.

In the beginning of October I wrote to my friend Dr. Koch, laying before him some of the impressions made upon me by what I had myself seen in practice since last June, when I began to follow this line of observation. I wished to know what he thought about certain points which seemed to me to be worthy of attention. Dr. Koch is not himself in practice, and, therefore, will give no opinion as to clinical matters. He told me, however, in reply to my letter, that the tendency amongst those hospital physicians with whom he had conversed on this subject, was in the direction of the belief that, in the future, the detection of the presence of bacilli will be of more importance than physical diagnosis, because it is a sure sign of the presence of tubercular phthisis.

As regards my own observations:—I have notes of 54 cases, and I hope, by and bye, to publish details of these and of other cases still to be observed. For the present, however, I think that my experience is far too limited for me to speak about it at any length. I will, however, venture to indicate what seems to me to be the tendency of the short clinical experience I have had. I think that we have now a method of investigating lung disease which, standing alone and unsupported by any other method of examination, throws a special light upon a patient's condition. This much I can already say, speaking from my own experience, that bacilli of tubercle are not always present in the sputa of patients whose physical condition would lead any one who knows something of Dr. Koch's work to expect to find them there. It is also a matter within my own experience, and it has happened to me more than once, to search the sputa for bacilli of tubercle and to fail to find them early in the history of a case of consumption. In the same case, within one or two months (for, as I have said, there were more than one such case) I have found bacilli in the sputum, and in one instance in enormous numbers.

My experience also inclines me to expect to find it established very shortly, that in the prognosis of phthisis we must look to this method of investigating the sputum for valuable information. I think it will be established that, given persistence of a large number of bacilli of tubercle in the sputum early in the history of a case, and that case will run a short course and end in death. On the other hand, I think it will also be established that, given few bacilli of tubercle in the sputum of a consumptive, and given also, that that condition of fewness of bacilli in the sputum characterises the case for some weeks, then that case will probably run a long course.*

---

* Since this paper left my hands, views similar to those indicated in the text have been published by Drs. Balmer and Fraentzel in the *Berliner Klin. Wochenschr.*, No. 45, 1882, so that their observations and mine appear, in some degree, to confirm each other. They speak, however, from a more extensive experience than I then had, and their observations go farther than mine.—G. A. Heron.

AN EXPOSURE OF THE CONTAGIOUS DISEASES ACTS, AND OF GOVERNMENT LOCK HOSPITALS.

BY ALEX. PATTERSON, M.D.

IN the *Glasgow Medical Journal* for last month, Mr. Lowndes, of Liverpool, a distinguished member of the society for the propagation of the Contagious Diseases Acts over the whole country, writes an elaborate reply to a few remarks I ventured to make in an article in connection with the Statistics of Glasgow Lock Hospital.

Whilst averse to enter into controversy with a professional brother on any subject, more especially on a subject so unpleasant, here the matter is one of such interest that I feel it to be my duty to give reasons for my belief. With regard to the excellence of the precept, *audi alteram partem*, I quite agree with Mr. Lowndes. A leading London medical journal, which for many a long year had this very precept as its motto, now declines to admit a single article in opposition to the Acts, and so declines, because it takes a different view of the question. The reciprocity is all on one side. With regard to the decrease or increase of prostitution in Glasgow, I may be permitted to state, that for thirty years I have been familiar with the streets of Glasgow by day and night, and without the slightest hesitation I affirm that they are immensely improved since 1870. Formerly there were large numbers of brothels in the city, some occupying two or three flats, and during the afternoon the inmates sat, often in a semi-nude state, at the windows, endeavouring to attract the attention of pedestrians, whilst at night the houses were brilliantly lighted up, and sounds of riot were to be heard at all hours. Solicitation at that time was so common, and so impudent, that in many parts of the city a foot passenger had to leave the pavement and take to the middle of the street. There are no houses of ill-fame of the foregoing description in the city now—or in the suburbs either, so far as I am informed on the best authority; and whilst solicitation still exists, it is of a mild description and rare, as compared with what it once was in Glasgow. Taking the great improvement in the streets, the diminution in the number of brothels, along with the remarkable decline in the number of admissions to the Lock Hospital, a fall which took place at once on the enforcement of the Police Act, I still hold the opinion that prostitution is not increasing in Glasgow and its suburbs, in proportion to the population, but the reverse. Now for the proof, and here *audi alteram partem* fits in conveniently. Dr. James R. Lane, a most respectable pro-Acts man, who appeared, I think, as a witness before the General Commission, and before the Select Committee last year, wrote a pamphlet in answer to a speech by Mr. M‘Laren, M.P., where he says, "Another mode of *showing very conclusively the diminution of disease* is afforded by the fact that a large number of the Lock Hospital beds have been for some time unoccupied. At Aldershot, with 100 beds, 40 were vacant in the middle of the present month. At Portsmouth, with 120 beds, only 84 are occupied. At Chatham, with 88 beds, only 63 are occupied." "This is *post hoc ergo propter hoc* with a vengeance!" or is it so, only when the argument tells against the Acts? Take Captain M‘Call's evidence (7,644-5), "It is a matter of fact, not mere conjecture, that the administration has reduced the number of prostitutes not residing in brothels." "I have no doubt that the number in Glasgow has been very much reduced in the ratio that the brothels have been reduced." Again (7,537), "I am persuaded, especially as regards young men, that taking away the temptation in the streets lessens the desire to go to these women." (7,413), "It has been alleged that these women have only been distributed and forced into more private resorts. All my enquiries and observations, however, are opposed to the truth of such a statement." Again (7,585), "I have not the slightest doubt in my own mind, as a police-officer, that the number of prostitutes in Glasgow and its suburbs has been reduced." Yet again, "The summary of thefts is instructive of the good which has flowed from the efforts made by the magistrates and police to minimise street prostitution and brothel keeping in the municipality."

From the year 1860 to 1869, the total number of informations lodged for street thefts, with or without violence, was, ...		5,067
Value of property stolen, ...	£24,446	
Thefts committed in brothels—informations lodged, . .		3,804
Value of property stolen, ...	16,843	
	£41,289	8,871

From 1870 (when the Police Act was enforced) to 1879, the number of informations of thefts was, . . .		2,887
The value of property stolen was, ...	£11,509	
Thefts committed in brothels—informations lodged, . .		808
Value of property stolen, ...	3,077	
	£14,586	3,695

The Registrar-General's tables show that in 1869 illegitimates were rated at 9·7; in 1870, 9·5; in 1871, 9·4; and in the last year it had fallen to 8·2, *i.e.*, from 9·7 to 8·2 in the ten years. There is no proof here of an increase of clandestine prostitution, rather the reverse, I am inclined to think.

The managers of the Magdalene Institution and Homes (Report, Dec. 1880), state that "This much needed repressive action on the part of our civic authorities has had the effect of partially protecting the virtuous, and making the practice of vice more difficult, while it has led to a desire on the part of many to be delivered from a life of evil," in proof of which the statistics are given—in 1860 the number of admissions was only 46; 1871 they mounted up to 114, and the number continued to increase in direct ratio to the vigour of the administration, reaching 244 in 1880. Captain M'Call gives the reason. "They find their calling is so hard and unprofitable now, that they are glad to take refuge in the institution." Then, again, when women seek admission to the Magdalene, they are examined, and if found to be diseased, are desired to go to the Lock Hospital and return when well. In 1872, 34·7 per cent were sent to the Lock. In 1880 the percentage of diseased had fallen to 18·03, showing a diminution of nearly 50 per cent of diseased, while the number of admissions had more than doubled. The decline in admissions to the Lock Hospital began in 1870, and was so marked that it could scarcely be attributed to any other cause than the enforcement of the police laws. Taking all the evidence combined, each individual part of which corroborates all the other portions, surely I was fairly entitled to draw the inferences which I did. If I am wrong, then the Lord Provost of Glasgow, the Magistrates and Council, the Registrar-General, the Managers of the Magdalene Institution, with Dr. Gray, their respected medical officer, the Managers of the Lock Hospital, Captain M'Call, and Professor Dunlop, most of whom have probably known the city from early life, are incapable of observation, or of forming a reasonable judgment—or, Mr. Lowndes alone is right.

I am pleased to observe that Mr. Lowndes and his colleague have found it necessary to exclude spectators, thus following the example of Glasgow, and that since this has been done the number of admissions has much increased. Mr. Lowndes here unconsciously uses a most powerful argument against the Acts—it proves that the females who resort to voluntary hospitals are not hardened, but that the great majority of them still retain their native sense of modesty. Farther on in the reply, Mr. Lowndes states that "The Acts are not directed against *women*, but against prostitutes." When have these prostitutes, taken care of by the Acts, ceased to be women? When they are registered as "Government women," and when they have become dead to all sense of shame. Then, if they are not women, what has so far altered them? The Contagious Diseases Acts undoubtedly. And what tends to keep them so for life? Again I say, the Acts. Then, admitting (which I do not) that the Government prostitutes *are not women*, what is the use of the moral and religious agencies which, we are asked to believe, form such an essential part of the Acts? It is said these women follow prostitution as a trade; it takes two parties to trade—and who are the customers? Where is the justice, I would ask, in any enactment, which punishes the one class, and that "more sinned against than sinning," while it lets the other go unpunished? No man is compelled to go into the harlot's company, while she, "one more unfortunate" ruined by him, and cast off by him, may have no other resource for a morsel of bread.

Mr. Lowndes fails to see how the Contagious Diseases Acts cause the "last remnant of modesty to be driven away." I should have thought proof of my statement unnecessary, and that reason would, with most people, have led them to that conclusion. However, as to proof of the hardening process—

Miss Brown, matron of Colchester *Government* Lock Hospital, said (*Q.* 17,965, *Roy. Com.*) "It hardened the girls very much. *They had a delicacy at first*, but latterly they went (to the examination) without any feeling." (17,969). "The feeling of privilege creates a bad effect on the mind." (17,970). "*The examination still more so?*" *Ans.*—"Yes."

Miss Webb, lady superintendent of the *Government* Lock Hospital at Chatham—*Question* 10,191, BB, '82.—"You would make a distinction, would you not, between the old offenders and those who are younger? *Ans.*—The old offenders seldom will turn." (10,193). *Ans.*—"*It is a hardening process, of course.*" *Q.* 10,202, Chairman—"The right honourable gentleman points out that year by year, as you go on, you find a heightening in the age of the prostitutes on the 'register,' which shows that there is apparently some influence at work which induces the women to remain on the streets a longer period?" *Ans.*—"Yes. I do not doubt that."

The Rev. Flavel Cook, D.D., chaplain to the Lock Hospital, Westbourne Park, which contains both classes of females. *Q.* 2,684—put by Mr. Osborne Morgan on behalf of the Government—"What distinction is there between the two classes?" *A.*—"The distinction is this, and I think it is obvious almost to any one at first sight, that the Government patients are horribly alike at one dull, dead level. The ordinary patients are very mixed, some of more and some of less refinement of appearance; but the *Government patients are brutalised beyond description in appearance and manner.*" *Q.* 2,754, Mr. Stansfield—"If I have followed your expression correctly, you expressed the opinion, with regard to these Government patients, that they were horribly alike?" *A.*—"Yes." *Q.* 2,755—"*Dull, dead, and brutalised beyond description?*" *Answer*—"Yes." Dr. Cook also stated that "there were words of Holy Scripture that were always coming into his mind about them—'Who, being past feeling, have given themselves over.'" This witness was brought by Government in support of the Acts!

The Rev. H. R. Baker, M.A., Vicar of Woolwich, says, in reply to *Q.* 2,087, BB, '82—"In fact, I should say, from my own experience, that it was almost impossible to reclaim one of the so-called Government women."

In answer to *Q.* 2,088, this gentleman says—"Another point that I should like to mention is this, that I believe myself most firmly that one cause of their returning to prostitution, and of the hardened disposition which they show, which prevents our being able to reclaim them as we should wish, *is the inspection.*"

Mr. F. S. Rimbault, town missionary in Maidstone for 28 years.—*Question* 3,317, BB, '82—"Upon what class of women would you expect reclamatory efforts to have the least effect —upon the registered or the unregistered?" *A.*—"I consider the registered girls are very hopeless, and that any attempts to reclaim them would be very hopeless."

Q. 4,377, BB, '82, Mr. Cooper, examined by Mr. Stansfield— "But you said that the elder and more hardened women willingly consent, because they have lost all trace of modesty in their composition, and they feel that the examination adds to the value of their profession?" *A.*—"Yes."

"The fact of this unremitting surgical manipulation of women, for the direct purpose of ever renewed vice, cannot fail to degrade women in their own eyes, and in the eyes of others, and must to that extent tend to suppress and override those moral scruples of which an invincible reverence for woman on the part of themselves, and on that of men, is the natural nurse and shield. This effect of the examinations becomes indefinitely intensified as they become more frequent, according to the persistent claims of the medical profession and the increasing practice, and is continued and aggravated enormously by every part of the system."—Sheldon Amos, M.A., Professor of Jurisprudence, University College, London. *Laws for the Regulation of Vice.*

Q. 4,460, Mr. Stansfield—"Is it not your view, therefore, that nine out of every ten of these personal examinations are practically made for the absolutely immoral purpose of ascertaining that the woman is apparently in a condition physically fit to conduct the business of a prostitute?" The Rev. J. P. Gledstone—"It is."

Proof enough on that point, I fancy.

Mr. Lowndes says, "Dr. Patterson does not make any allusion as to the state of the women on their admission—whether they are severely diseased or not." I have not, in the article to which Mr. Lowndes replies—but in answer to *Q.* 12,224, B.B. 07, he says, "I have read that evidence." Portions have possibly escaped notice, as *Q.* 2,965 will show that "the worst cases we get are from the rural districts, and those remain out, I suppose, as long as they can keep their feet to earn their bread; but those that are in the town, the moment they know they are infected with disease, I believe, come to us." In answer to *Q.* 12,250, Mr. Lowndes in his answer says, "I could not help observing that Dr. Patterson did not give the number of males admitted in Glasgow." In answer to *Q.* 2,886, I do give the number of males admitted in Glasgow. I should respectfully recommend a little more accuracy of statement.

As to giving Chloroform.—It is very seldom given in the Lock hospital for examination—eight or nine times a year perhaps—and, as regards that, we do what we consider best for the interest of the patient and of the institution with which we have the honour to be connected. If I can save the funds of an hospital by shortening the residence of the patients, I consider that I am justified in so doing, and as for our results, see the *Lancet* for 6th January, 1882.

"This is the reason (the mildness of the disease (?) in the Queen's women), says Mr. Lowndes, why those who support these Acts urge the importance of periodical medical examination and compulsory admission to hospital at once of all women found to be diseased—not to prevent men from the consequences of vicious indulgence, but to save these unfortunate women themselves from untold suffering." The Acts were passed, presumably for increasing "*the efficiency of the Army and Navy*," and this efficiency, it was supposed, *would be obtained by keeping women free from disease, that soldiers and sailors might indulge their propensities with the delusive* *hope of escaping disease.* It does seem to me a strange method of saving unfortunate women from disease, to register them and make prostitutes of them for life.

Mr. Lowndes says that those women would make their way to a camp established on Ailsa Craig or on the Bass Rock; that I admit, if boats were supplied by the Admiralty—they would go where they find customers. The men will do very much, infinitely worse—for example, Mr. G. W. Romaine, who was Under Secretary to the Admiralty, gave evidence before the Lords on the Acts in 1868, and before the Commons in 1869, says, in answer to Dr. Brewer, *Q.* 956, "It was found, even in the South Seas, that one of the vessels (men-of-war) would go to an island in the Pacific, *where disease had been utterly unknown*, and you immediately had an outbreak of disease." Now, so far as these unfortunate islanders were concerned, surely the men were the delinquents. Was this visit a blessing or a curse, or would it be termed civilisation? A very old name for syphilis was French Pox. Now, does it not seem probable that a similar event occurred in reference to our own islands, and that our Gallic neighbours brought in the disease? and here again men were likely to have been the offenders.

Evidence of Dr. Ross before the Royal Commission, that when a regiment goes to the East Indies, "so many prostitutes—with more in reserve if wanted," are officially ordered by the medical officer; and the order is countersigned, as a matter of form and of course, by the commanding officer.

With regard to the Hebrews, Mr. Lowndes may accept it from me, that I am aware of what circumcision may do in the prevention of infection. There is another reason referred to in my paper, *i.e.*, their sobriety, which Mr. Lowndes has overlooked, and which does more to prevent syphilis than all the Contagious Diseases Acts in the world. On the 30th December, 1882, there appeared in the *Lancet* an unique paragraph, in which it was stated, "that during the year 1882, in the town of St. Ives, Cornwall, with 7,000 inhabitants, there were only two cases of drunkenness brought before the court, and the whole police force consists of one man."

I thought it would be interesting to learn to what extent prostitution was found to be a necessity in St. Ives, and wrote the mayor, who, in a very courteous letter dated 11th Jan., says:

"DEAR SIR,—I feel highly gratified that my letter to the *Alliance News* has produced such a feeling throughout the country. The marvellous state of things here is entirely owing to the practice of total abstinence by a majority of the inhabitants of this town for nearly half a century. . . . With regard to your enquiry respecting prostitutes, I have enquired at the policeman (!), and he informs me that *he does not know of one prostitute in the town, and there is not one brothel in the place.*

"There cannot be a doubt that drinking and prostitution go hand in hand. . . .—I remain, yours faithfully,

"JOHN M. KESWICK.

"DR. PATTERSON, Glasgow."

This letter proves that prostitution is not a necessity anywhere, it proves that where men are sober they may be virtuous, and that where men are virtuous, women will be chaste. It also proves clearly to my mind the true way in which venereal diseases may be diminished. Many of our rural districts, thirty or forty years ago, were as free from vice as St. Ives happily now appears to be.

Mr. Lowndes says, "With regard to the ages of those whom Dr. Patterson calls "Queen's women"—pardon me. They are called so over and over again by witnesses on both sides of the question. Royal Commission, Mr. Wolverstan, surgeon, Devonport, 23,434-36. Miss Bull, matron, Government Lock, Devonport, Q. 7,863, 7,894, 7,896. Mrs. Lewis, a Biblewoman, Q. 12,622-12,643. They are termed "Government women" on all hands. And in a speech delivered in the Kinnaird Hall, Aberdeen, 24th March, 1871, the following sentences occur:—"What doctrines these chaplains can preach I know not, but I know what doctrine *they cannot preach*—the doctrine that bids the sinner 'go and sin no more.' It is idle to tell us that this is not licensing harlotry. It is manifestly so. A house of ill-fame is only prosecuted if harbouring a woman *not* subjected to this law; and a prostitute, if subjected to this law, is a national institution supported by the taxes of the virtuous. Already, in the subjected towns, the most abandoned women proudly call themselves 'Government women' and 'Queen's women,' and boldly assert that their infamous traffic is legalised; and with justice do they say so. What is this but making harlotry one of our national institutions?"

Ages of the "Queen's Women" and the Hardening Process.—Of the latter I think proof enough has already been afforded. Paragraph from Majority Report of the Select Committee—"This is attributable, not to the so-called hardening effect of the Acts, which have proved powerful for purposes of reclamation, but to the improved health of the women and their freedom from neglected disease, which in former times cut short not only the career but the life of the prostitute at an early age."

Well, we shall see. The late Mr. Acton says that, "When the prostitute withdraws from her career, as withdraw she will, for they last as long as other people, and die at last of maladies common to respectable humanity, it is extremely rare to find one with her nose sunk in, palate gone, or nodes on the skin." This is undoubtedly true of the prostitutes in unprotected districts. Duchatelet, who had an amount of experience in Paris, which, fortunately for our country, no one has had here, says, "Notwithstanding their excesses and exposure to so many causes of disease, their health resists all attacks better than that of ordinary women who have children and lead orderly lives." This may be accounted for by the fact that the majority of them in early life fall victims to the seducer, on account of their handsome figures and healthy appearance. In fact, with many a one, "Her very beauty is her only earthly dower." Dr. Drysdale says, "It used to be presumed that prostitutes lived a few years of sin and misery and then died; but this view has not been found to be in unison with facts. To sum up what will be vouched for by observers in all cities, the health of prostitutes is *above the present standard* of female health, the only disease peculiar to them being venereal diseases. It is a popular error to suppose that these women die young or make their exit from life in hospitals and poorhouses."

The late Mr. Holmes Coote—"It is a lamentable truth, that the trials which respectable, hard working married women of the working classes undergo, are more trying to the health and detrimental to the looks than any of the irregularities of the harlot's career." Mr. Coote was a member of the Commission when the Acts were introduced, and when he saw the official working of the system he became a determined opponent of the Acts, and a member of the Association for their Repeal.

"*The improved health of the women!*" "*The disease is milder in form! This proves that hygienic purposes are better served by the presence of a reduced number of prostitutes, liable to increased risks of the recurrence of disease in a mild form, but subject to careful medical treatment, than by the presence of a large number of prostitutes in a chronic state of neglected disease!*" From Majority Report, which hitherto I had ignored; and I have to thank Mr. Lowndes for drawing my attention to such a remarkable document.

Does an increase in syphilis among the women mean "improved health?" If so, then the Acts have improved their health in a very peculiar manner.

There is more syphilis amongst women in the subjected than in the unsubjected stations. Take proof: Q. 1,800. "BUT I UNDER-

STAND YOU TO SAY THAT THERE WAS MORE SYPHILIS AMONGST THE WOMEN?" Inspector-general Lawson answers, "YES."

Q. 1,804. "*A man runs more danger of catching true syphilis in the subjected stations than in the unsubjected stations!*" Inspector-general Lawson answers, "TO THE EXTENT OF 36 AGAINST 33!" And in answer to Q. 1,803 the same high authority answers, "YES, THAT IN 100 CASES YOU HAVE 36 CASES OF PRIMARY SYPHILIS AT THE SUBJECTED STATIONS, INSTEAD OF 33 AT THE UNSUBJECTED STATIONS."

Increase in proportion of secondaries to primaries in the stations under the Acts. Q. 1,817. "INCREASE IN SUBJECTED DISTRICTS, 18·6." Q. 1,818. "IN THE UNSUBJECTED DISTRICTS YOUR INCREASE IS ONLY 11?" Inspector-general Lawson. "THAT IS SO.

This, then, it appears, has been the effect of the Acts on the prostitutes. And it has increased the risk of the men's catching disease. So that the Acts have had the very opposite effect to what was delusively expected. Clearly the Acts are a great mistake, for the very evident reasons shown. The women have much less disease in the unsubjected stations, and in them Inspector-general Lawson admits that the men run less risk.

At Devonport the disease among registered women has risen continuously from 102· per cent in 1875 to 171· per cent in 1880.

Aldershot is most remarkable. A place where, *if there were anything in the Acts as to prevention*, the results should be beyond question. Quite the contrary. Between 1870 and 1880 the average number of women fell from 326 to 150, while the ratio per cent of diseased inversely rose from 206 per cent in the former year to 356 per cent in 1880. During the whole of the latter period a large number of women annually "left the district," amounting on an average to nearly one-third of the total number on the register. The percentage of disease to the actual number on the register in 1880 was higher than it had ever been before, *i.e.*, 194·55 per cent, and of course the risk to their male associates will increase. Does all this evidence prove "that hygienic purposes are better served by the presence of a reduced number of prostitutes?" I assert most distinctly not, and the proof is from Government witnesses of the highest authority. I had almost forgotten to give Dr. Barr's most experienced opinion on this point; curiously enough, it is directly antagonistic to the assertion of the select majority. Dr. Barr, in answer to Q. 1,855—"I stated this, and I state it again, that in consequence of having so small a proportion of women to men, there is more disease propagated than if there were five or six times the number of women."

Mr. Lowndes says—"It is unfair to compare the prostitutes of Glasgow with those just described." I did not do that. At the preceding page, Mr. Lowndes says—"It matters little what they call themselves, so long as they are, to all intents and purposes, prostitutes." I think it does; it points to the fact that they are ashamed of their calling, and also that the immense majority of them get tired of it, and abandon it, whilst the tendency of the Contagious Diseases Acts is clearly to keep them prostitutes for life. There is much juvenile prostitution, I admit, and that one must always regret, and hope that by and bye it may be lessened by some moral means—means which may affect both sexes alike. As to its being completely eradicated in the Acts' districts there is abundant proof to the very reverse; in fact, it is bound to increase in all countries where such Acts are in force, and for this reason, that very soon men become acquainted, by dear bought experience, that they run much greater risks with licensed prostitutes, and seduction becomes more common. The records of the Rescue Societies in the Acts' districts totally disagree with Mr. Lowndes as to the complete eradication of juvenile prostitution. A petition was presented in July, 1879, to the House of Commons. It is a petition of ministers of religion and other residents in the town and neighbourhood of Southampton, and it shows — "That your petitioners reside in the town and neighbourhood of Southampton, which is subjected to the operations of the Contagious Diseases Acts relating to women, and are well acquainted with the district. That your petitioners have had their attention called to the yearly report of the Contagious Diseases Acts' Police, and amongst other things, to the following figures:—That in Southampton, which has a population of nearly 54,000 persons, there were in the year ending 31st December, 1876, only one common woman under 18 years of age, and only four between the ages of 18 and 19, and that in December, 1877, there were only two such women under 17 years of age, and six between the ages of 17 and 18; and that at the close of 1877 there were only ninety-eight such women of all ages under and over 31 years of age. Your petitioners are shocked to find that these *absolutely unreliable statements* have been placed before Parliament, year by year, in an official return on the operation of the Acts, bearing the signature of Captain Harris. If it had been stated to your honourable House, in the aforesaid return, that for humane and prudential reasons the police refrained from putting juvenile prostitutes on the register, and submitting them to the demoralising influences of the Acts, your petitioners would have no reason to complain. But they observe that the object of the figures referred to is to lead your honourable House and the country at large to believe that juvenile vice is being stamped out by these Acts." *Q.* 5,567—"That contests the accuracy of the figures given under Captain Harris' report, and treats them as grossly incorrect I may say?"—"Grossly incorrect." This petition was signed by several clergymen and magistrates—by W. B. Randall, J.P., Chairman of the School Board, by Major-General Tryon, by W. C. Westlake, J.P., by Edwin Heard, J.P., John J. Tucker, J.P., and many others.

Dr. Wolferstan of Plymouth—"My opportunities of forming an opinion have been ample. I was for five years resident medical officer at the Royal Albert Hospital, in which all the women subjected to the Acts have been detained for treatment. During this time about 1,500 women, representing in round numbers 4,000 admissions, were brought under my immediate notice. I am opposed to the present Acts, because I believe they have failed to effect any material improvement in the health of the soldiers and sailors; *they have greatly increased clandestine prostitution, and with it disease among the civil population, and illegitimacy.*"

The following statement, relating to the condition of Portsmouth and Chatham, is obtained from Captain Harris's returns to the Government, and shows how baseless are the assertions that the Contagious Diseases Acts have reduced prostitution in these towns, which have been so long under their operation; or that these Acts have produced the moral results so often attributed to them by their advocates.

"Returns for 1877.

		AUTHORITY AND REFERENCE.
"Number of women registered for the *first* time during the year." (Presumably, therefore, women who had not been prostitutes before.)	The number was larger by 105 than the number in 1876. In 1876 there were 295; in 1877 there were 400.	Harris's Report for 1877, page 6, col. 1, Portsmouth.
Continued in prostitution to the end of the year, and were re-registered at the beginning of 1878.	The number was larger by 56 than in 1876. In 1876 there were 476; in 1877 there were 532.	Ditto, page 6, col. 15.
Diseased prostitutes sent to Hospital.	More by 155 in 1877 than in 1876. There were 477 in 1876, and 632 in 1877.	Ditto, page 7, col. 21.
Ratio of disease per 100 women on the register.	This has risen 27·3 p. cent, being 95·4 per 100 women in 1876, and 122·7 per 100 in 1877.	Ditto, page 7, col. 31.
Returned to prostitution immediately on leaving Hospital: the practical test of the boasted moral benefits derived from the C. D. Acts.	More by 129 in 1877 than in 1876. In 1876 there were 430 who immediately resumed prostitution. In 1877 there were 559,	Ditto, page 7, col. 23.
Entered "Homes," which are neither provided nor assisted by Government or the Acts.	Fewer by one-twelfth in 1877 than in 1876. In 1876 there were 49, and in 1877 only 45.	Ditto, page 6, col. 12.

"There are also at the present time several women awaiting their trial for breaking windows in the Portsmouth Hospital, where such exceptional pains are said to be taken for the moral reformation of the inmates who are compulsorily detained there."

It appears, therefore, from the latest information relating to Portsmouth, that the present condition of things is by no means of a roseate description, for—

"*The number of Prostitutes registered for the first time* is higher than it has ever been during the last nine years.

"*The number of women who have immediately returned to prostitution,* after having enjoyed the moral benefits of the hospital, is higher than it has been for the last six years, and—

"*The proportion of Venereal Disease among the registered women* is higher than it has been during the last ten years.

"CAPTAIN HARRIS'S RETURNS FOR CHATHAM FOR 1877.

"Chatham, like Portsmouth, has been loudly praised for the beneficial results of the Acts. We shall therefore examine it as we have done in the case of Portsmouth.

		AUTHORITY AND REFERENCE.
"Number of women registered for the first time during the year."	The number was larger by 79 than the number in 1876. In 1876 there were 122, and in 1877 there were 201.	Harris's Returns for 1877, page 6, col. 1, Chatham.
Continued in prostitution to the end of the year, and were re-registered at the beginning of 1878.	More by 23 than in 1876. In 1876 there were 147, and in 1877 there were 170.	Ditto, page 6, col. 15.
Number of brothels known to the police.	Has increased from 64 in 1876 to 72 in 1877.	Ditto, page 17, col. 15.
Diseased prostitutes sent to hospital.	More by 140 than in 1876. In 1876 there were 272, and in 1877, 412.	Ditto, page 7, col. 21.
Ratio of disease per 100 women on the register.	Increased by 88·5 per cent. In 1876 it was 165·8, and in 1877 it was 254·3.	Ditto, page 7, col. 31.
Returned to prostitution immediately on leaving hospital, notwithstanding the alleged efforts at reclamation and their asserted success.	More by 118 than in 1876. In 1876 the number was 252, and in 1877 it was 370.	Ditto, page 7, col. 23.
Entered "Homes," not, however, provided or assisted by the Acts or Government.	One more than in 1876. In 1876 nine women out of the total number of prostitutes (344) entered Homes. In 1877 ten women out of a total of 397 did so.	Ditto, page 6, cols. 9 and 12.

If the examination is carried still farther back than 1876, we find that—

"*The number of Prostitutes registered for the first time* is higher than it has been for the last eight years, with the exception of 1874, which was ten higher than 1877.

"*The number of women who immediately returned to prostitution* after having enjoyed the moral benefits of the hospital is higher than it has been for the last eight years, with the exception of 1872, which was higher by 25 than 1877, and—

"*The proportion of Venereal Disease among the registered women* is higher than it has been for the last nine years.

"*The number of women sent to hospital diseased* is higher by 140 than in 1876; and, at the same time, the number of women who have left the district unaccounted for, and probably carrying disease with them, is higher by one-sixth than it was in 1876."

Mr. Lowndes states that the "Acts do not recognise prostitution as a necessary evil, though they do recognise, as the Glasgow Police Act does, that prostitution is an existing fact." Let us see what General Sir Henry Storks, one of the highest authorities in support of the Acts, says:—(Lords Repeal, appendix, p. 132.) "In conclusion, I am of opinion that very little benefit will result from the best advised means of prevention *until prostitution is recognised as a necessity.*"

Q. 5,981, BB., '82, Mr. Hanson, examined by Mr. Stansfield—"You are not, I take it, able to say what is the cause of this change of view on the part of the military authorities?" *A.* "I can see no other cause for it, except that the fact of the women being examined, *recognises a necessity of the soldiers consorting with prostitutes.*" The witness had been in the army for 25 years.

Q. 6,117, previous witness examined by Mr. Osborne Morgan—"But surely there is not so much in the army as there was, is there?" *A.* "I think there is more."

Q. 6,118—"Are you aware that there is evidence the other way?" *A.* "I have not seen any evidence as to that. I know that in my regiment the boys of late years went out with prostitutes; *they did not in the old times.*"

Q. 6,121—"You would go so far as to say that there is more vice now in the army than there was in former days?" *A.* "More of that particular kind."

Q. 6,122—"Notwithstanding the decrease of drunkenness and the increase of education?" *A.* "*I do not believe it is so much regarded as a vice or as a sin as it used to; and so it has spread.*"

The men regard prostitution as a necessity, and defend their opinion rightly by saying that it is sanctioned by authority of Government, that there are surgeons provided to inspect the women, hospitals provided, and all these things are done under Act of Parliament passed for the purpose, and supposed to have been passed solely for their benefit.

Take also the evidence of Dr. Ross already given—"So many prostitutes, officially ordered, with more in reserve if wanted." Surely that admits the necessity of prostitution. By the way, in India, over the houses of this description a single numeral is placed as a sign, with a tariff of 6d. In Pompeii the little god Priapus was exhibited in front of similar establishments. How closely the one resembles the other! Are we then going back to heathenism?

Take *Q.* 3,348, put by Dr. Farquharson, a strenuous supporter of the Acts—"Here, I suppose, the increase in the number of prostitutes would be just sufficient to meet the wants of the increased population of the town of Maidstone, &c." This shows the gentleman's opinion as to the supposed necessity of vice. The good people of Maidstone will feel obliged to Dr. Farquharson no doubt.

The opinion of Dr. Barr, of Aldershot, is, "that it is the interest of the public service that up to a certain point prostitutes be multiplied, and that it is the duty of the military surgeon to co-operate in vice being rendered as innocuous as possible, that is, in other words, to facilitate it." It has been clearly proved that the Acts have failed to render vice safe—exactly the opposite. This is precisely what state paid hospitals, as shadowed by Mr. Lowndes, offer to do for the whole country. It is for the people to say what they think of such a generous proposal. *If Government is of opinion that prostitution is wrong, then why does Government license it? If the Acts license it, as they clearly do, Government must think it a right thing, and by doing so admits that prostitution is a necessity.*

A few years ago a memorial was presented to Mr. Gladstone by Mr. Samuel Morley, and signed by 885 Congregationalist ministers, from which I take the liberty of quoting a few sentences—"If the State, at vast public cost, prepares a great highway, provides a special department to keep it in repair, and assures men that it is safe, it is idle at the same time to try to prove that frequenting it is an act of tresspass." "Your memorialists further submit that there are two sorts of recognition of any existing evil by the State. The *law recognises other evils by punishment*, or by *attempting to put an end to them (as the Glasgow Police Act does)*. The law in these Acts *recognises prostitution by regulation*, and by *affording assistance to continue it with impunity*, which your memorialists submit gives a direct sanction to profligacy, and threatens disastrously to confuse the moral sense of the nation."

Mr. Lowndes quotes the 12th clause of the Act of 1866 as to adequate provision being made for the moral and religious instruction of the women detained under the Act. This clause is brought prominently forward by all those who support the Acts, and held up as being the great difference between the late French Acts and the English ones.

In the St. Lazare and the Lourcine Hospitals in Paris, Sisters of Mercy undertook the religious duties. What, for instance, can those fallen women think of the genuineness of a religious or moral instruction afforded by the chaplains, when the inspecting medical officer "makes it a rule to supply them with a syringe and lotions at our expense, and instructs them how to use them, so that they may keep themselves and the soldiers clean." I speak with all deference to gentlemen who hold an opposite view, but I cannot see how religion can be associated with Acts which, in the long run, are subversive of religion and order in all places. If we teach men to give up all respect for Divine law, we may be certain they will not long obey laws made by man.

"Dr. Patterson's seven objections are soon disposed of." So says Mr. Lowndes. Absurdity of secluding one sex only in disease common to both. There is a great deal of writing and speech making going on at present regarding the notification of infectious diseases, and what would our respected sanitary chief, Dr. Russell, think of an enactment which gave him power to suppress scarlet fever, and at the same time told him to look after and seclude the female children only? What would veterinary surgeons say if you required them to stamp out the cattle plague in this manner?

"2, 3, and 4 are all matters of opinion, the result of practical experience." Are they so really? In Lancereaux's work, vol. ii, the subjoined table appears. It is tabulated from M. A. Fournier's (assisted by M. Puche) thesis on "Syphilis." Fournier is the successor of Ricord:—

Males Infected by	Public Prostitutes, . . .	625
" "	Clandestine Prostitutes, . .	46
" "	Kept Women, Actresses, &c., .	52
" "	Work Women, . . .	100
" "	Servant Women, . . .	26
" "	Married Women, . . .	24
		873

Thus, out of 873 cases coming promiscuously under the care of these gentlemen, 625 contracted syphilis under the temptation of a false security, from women warranted clean for safe usage by a paternal government, whose example we are asked to imitate.

Dr. Vintras, examined before Venereal Commission, says—"You will find almost all chancres are, in Paris, derived from women who make prostitution their sole business"—registered and regularly examined prostitutes.

Armand Després, for many years attached to the Lourcine in Paris, says—"I have closely examined these contagious maladies, and I have satisfied myself of the *absolute inefficacy* of the measures adopted by the Police to prevent their propagation."—1877.

Diday of Lyons says—"Every day I see some unfortunate people infected in the first class houses, which, besides the official inspection, pay for the luxury of a doctor to attend the establishment. . . . Led by a very plausible reasoning, many people watch the coming out of the dispensary of the woman who has just undergone inspection. . . . It is not unfrequent to observe cases of the most aggravated kind of disease, caused by such contact, guaranteed as free from risk, by Government."

Dr. Berkeley Hill, Secretary of Association for promoting the Acts, says—"I was informed that on the day after inspection, the houses are specially frequented by the public, in the belief that there is then less chance of contracting disease. It is therefore reasonable to suppose that many yield to indulgence whom the fear of the natural penalty would otherwise deter." We now know how much greater the chance is of contracting the disease in the subjected districts. This statement is valuable, as coming from an enthusiastic supporter of the Acts, who thus proves that the Acts promote prostitution, and consequently increase disease.

Mr. John Simon says—"The various local states which *most habitually* spread the infection of true syphilis are *constantly overlooked* in examinations *made expressly* for their discovery."

The *late Mr. Acton* says—"Notwithstanding all assertions to the contrary, the best managed regulations cannot guarantee freedom from disease."

Mr. Berkeley Hill—"There is great difficulty in detecting disease if it is the interest of the prostitute to conceal it, and some disease must always escape detection."

Mediate Contagion.—Now, I desire the reader's especial *attention to this deliberate statement of Mr. Lowndes*, and leave him to form his own opinion as to Mr. Lowndes'—accuracy. "Mr. James Lane of the London Lock Hospital, Dr. Barr of Aldershot, and other experienced examiners, consider the *probability* of *mediate contagion as very* remote."

Mr. Lane—"I think it is impossible to deny the possibility of mediate contagion." (2,395). "It is quite possible, if a woman has connection with a great number of men (or any lesser number), and adopts no means of cleanliness, . . . but my knowledge does not enable me to say how infrequent it is, nor can any man's knowledge enable him to say how very infrequent it is." (2,529 to 2,546).

Dr. Barr of Aldershot.—The great spread of disease in this manner is frequently referred to by Dr. Barr. *A.* 1,482 —"*A woman at the time is free from disease*, and the *fact of her communicating venereal disease to a soldier is not due to her unfitness, but to contagious matter which she retains within her, the result of intercourse with a diseased man.*" (1,481-1,795). "Gonorrhœa, syphilis, and sores are communicated in that way, the woman herself remaining free."

Question 4,738 B.B.—"We have heard about 'mediate contagion.' You are aware that that is a very controverted point in the profession?" Dr. Barr answers, "I have heard that it is so. I have my own opinion about it. I BELIEVE IN IT FROM ACTUAL PROOF." In answer to question 4,742 Dr. Barr answers, "Certainly. In this matter of mediate contagion, I am speaking of a woman having had intercourse with twenty different men, and not cleaning herself after connection. I say it is only fair to assume that, after having connection with a lot of these men, although she may be healthy, they being diseased, subsequently might disease a young recruit; *that is, by mediate contagion.* I SHOULD NOT HAVE ADVANCED THIS STATEMENT UNLESS I COULD PROVE IT." When it is remembered that Dr. Barr has spoken of this almost incredible amount of intercourse "in the limited portion of the day that soldiers are away from barracks, it will at once be evident what an amount of disease is communicated in this manner; in fact, one man may thus infect a dozen men. Then, no surprise will be felt that the rate of improvement among the men at Aldershot has been reduced from 6·6 per cent before the Acts to 2·6 per cent annually since then" (1,792). Then what can be the use of examining these women at any time? Any one of them might be examined and pronounced free from disease, admittedly so by Dr. Barr, a very experienced examiner, and yet communicate disease to all comers. And I have not the slightest doubt that this occurs constantly in all State licensed and State paid examination brothels in the world; not to the same extent numerically, possibly, yet sufficiently so to spread disease in all directions. Dr. Barr is perfectly correct; his evidence is very plain, but honest—and it is a fact well understood by some old soldiers. It appears to me to be one of the links of sand in the chain of the Acts in all countries, and to my mind proves incontestably, not only the utter futility of all such laws, but that human ingenuity could with difficulty have devised an arrangement more likely to spread prostitution and disease than the Contagious Diseases Acts.

Mr. Lowndes says, "The probability of latent disease being undiscoverable, is only an argument in favour of examination at more frequent periods." Well, let us see how frequent some very experienced examiners think the examinations ought to be made, then my readers may conjecture with what frequency Mr. Lowndes would examine. In reply to *Q.* 2,744—"I think it was your opinion, upon a former occasion, that the disease was propagated by women in *spite of fortnightly examinations?*" Mr. Lane answers, "It must be to a certain extent." *Q.* 2,745—"How far would you carry the frequency of the examinations to be safe?" *Ans.*—"I think that, practically, you can hardly make it more frequently than fortnightly." *Q.* 2,746—"It is to be admitted, then, that under the present system disease is propagated between the times of the examination?" Mr. Lane, "Of course it is." Mr. Lane proves that infection may take place at any time between examination periods, and Dr. Barr proves that a great many men may consort with a single woman in the course of an afternoon and night. Can anything but an increase of disease be expected under such circumstances?

The late M. Ricord thought prostitutes ought to be examined not less than every three days; M. Ratier and M. Sandonville every four days; M. Davilla, M. Langlebert, and many others besides, twice a week; M. Lancereaux, every two days. "In the case of a woman who has once been treated in an hospital for syphilis, Dr. Mireur himself recommends an examination either every day, or at least every other day, and this for eighteen months or two years after leaving the hospital"! *Amos*, p. 45.

Mr. Lowndes says, "The infecting power of the secretions of a syphilised woman has yet to be proved, and presents no difficulty to the periodical examination of prostitutes." I am afraid this is not an accurate statement. At the International Congress in London in 1881, Professor Henry Lee, one of the highest authorities in the world on the subject, appeared and spoke—"I have here the drawing of some *inoculations* made from the *mucous secretions of a syphilitic patient* upon another patient also syphilitic. *There was not the slightest appearance of any syphilitic disease in the part from which the syphilis was taken*, and the greatest care was exercised by M. Gascoyne and myself in performing the experiment. The inoculations showed a decided increase of substance, and resembled boils passing into a state of suppuration. The appearances produced were in these respects quite different from the sharply defined ulcerations which result from inoculations of the local venereal sore." The drawing illustrates two points—

1. The character of the inoculations produced by the inoculation of syphilitic matter upon a syphilitic subject; and 2. The inoculability of the secretions of a syphilitic subject, taken from a part where there is no apparent syphilitic disease.

This *experiment shows how readily the mucous secretions of a syphilitic patient may be the means of communicating disease*, and throws great light upon Hunter's celebrated experiment, when he inoculated himself with what he considered to be the gonorrhœal discharge of another person. The result in Hunter's case was a primary sore, which in its turn was followed by secondary symptoms which lasted some three years. I may state that Professor Lee's experiment has been repeated. Most of us are familiar with the fact that a man having contracted syphilis, gets apparently cured; remains free from any appearance of the disease, may be for years; gets married, and disease makes its appearance in his wife and offspring. Why, then, may not the reverse take place, and the syphilised woman give it to the man? Professor Lee goes on to state "that as far as the prevention of disease is concerned, these Acts appear to have been framed upon the supposition that, by a local inspection, a person could be pronounced either contagious or free from disease. This, however, has now been demonstrated not to be the case; and I should say that *fully one half*, if not more, *of the cases of syphilitic infection are derived from the secretions of patients who have long ceased to have any primary sores.*"

Mr. Lowndes states that Inspector Anniss' reduction of brothels in his district to 70 was strongly disputed by the opponents of the Acts, this reduction to 38 (in Glasgow) has been accepted without the slightest hesitation. Inspector Anniss' statements were disputed, and rightly so, because "the duty of suppressing brothels does not form any part of the operation of the Contagious Diseases Acts, or of the appointed work of the officials employed under the Acts. They only impose a penalty for harbouring a *diseased* prostitute, and have no legal power to reduce brothels. Here is the proof that Mr. Anniss's statements were with justice disputed. Devonport is in the very centre of the subjected districts, and on the 12th October 1882, the Town Council met and agreed "that the *answers given* by *Inspector Anniss* to questions as reported in the *Parliamentary Blue Books* of 1881 and 1882, containing the examination of Anniss by the Select Committee of the House of Commons, are *in a very high degree misleading*, some of them *highly coloured*, and *others not strictly true*. . . . And that, whilst Inspector Anniss takes the credit, as an officer under the Contagious Diseases Acts, of being the *sole means of closing several public houses and beer houses in the borough, which were said to permit prostitution therein, the sub-committee are persuaded that on* NO OCCASION *has he ever interfered, by way of information or complaint, with a view of procuring a conviction against any one of the keepers of such houses*, but on the contrary, they are satisfied, from recorded facts set before them, that the several closings have been in consequence of the action taken by the local police, under the superintendence of Mr. Superintendent Lynn." Whilst the members of Council refrain from giving an opinion concerning the Acts, it appears to be pretty evident, from the tone of the statement, that they think Devonport could do without them. On page 19 of the Police Report for 1881, delusively headed, "On the Operation of the Contagious Diseases Acts" a reduction of 140 and 260 beer house brothels is claimed to have been effected by the Contagious Diseases Acts' police in all the towns where the Acts are in force, from 1866 to 1881, inclusive. Mr. C. Maclaren, M.P., for Stafford, asked for a supplementary return, covering the same years and the same districts, to show whether these immoral houses had been closed by the local police, or the metropolitan police, who carry out the Contagious Diseases Acts. This return (No. 375, session 1882), shows on page 32 that during the sixteen years of the Acts, and in all the places where they are in force, *including the district for which Inspector Anniss is responsible*, the Contagious Diseases Acts' police have closed *One* public house and *One* beer house! Neither of these two houses was proceeded against by Anniss for being a brothel, but the public house was dealt with for harbouring a diseased prostitute, and the beer house for harbouring a naval deserter. The return is signed H. C. Cutbush, chief inspector; D. Labalmondiere, assistant commissioner. So that there were grounds for disputing some of the statements referring to the closure of brothels. In fact, closing those houses would not answer, according to leading supporters of the Acts, because it would tend to diminish the "supply."

Mr. Lowndes says, "The laws in Paris which related to prostitution were so totally different to our Contagious Diseases Acts, that it is quite incorrect to term them so." With all respect I assert most emphatically that the laws are identical in spirit and identical in principle, and when closely inspected the organisation is similar. The English Acts virtually license the brothels when they permit them to exist as brothels, and only interfere with them when there are diseased occupants—the females are "registered" as common prostitutes. Surgeons paid by the State make fortnightly examinations for the express purpose of seeing that they are fit to pursue their calling—in Paris it was done weekly or more frequently at times—and the State pays medical men and supports Lock hospitals that they may be cured. It will be observed that, in our colonies, the French system is carried out to the letter at the stations. I am not aware that there are chaplains attached there to give religious instruction. If I may so express myself, the Acts are a French tune with English variations. The moral and religious element is held up as the great difference. The organisation was more thoroughly worked in Paris than it conld ever be here; yet, after a hundred years' sad experience, it has been given up; and I think there are few of our honest French neighbours who would not say that it would have been very much better for France during the last hundred years if such laws had never existed. Dr. Barr, when asked what class of women were brought under the Act by the police, replies, "Milliners and dressmakers, labourers' wives, the wives of small tradesmen, and domestic servants *of course*," and these are registered. Supposing for a moment that it were possible to introduce those Acts all over our country, what would be the results? The women of these islands would never submit to such brutalising proceedings; and even as it was in Paris, with all their despotic regulations, not one in ten could be found to submit. In 1875 the Police of Paris were able to bring under the regulation system 3,853 out of a total of suspected prostitutes of from 30,000 to 50,000! And in Berlin only 1,650 out of 26,956. In London there may be 60,000, and would we fall any better with them? Certainly not. "Whatever renders vice apparently (although never really) safe, increases its prevalence, and must necessarily increase disease."

The subjoined table is both interesting and instructive:—

TABLE showing the relation of Illegitimacy to "Regulations" of Prostitution.

PLACE.	CONDITION OF PROSTITUTION.	PROPORTION OF ILLEGITIMATE TO TOTAL BIRTHS.		AUTHORITY.
		Illegit.	Per	
England generally	Regulated in 16 towns. 1-30th of the whole population, . . .	5·8	100	Registrar-General's Report, 1878, p. xxi, average for 24 years.
France generally .	Regulated in Paris and some other large cities	6·98	100	M. Sautter de Blonay, in Bulletin Continental, 1876.
Prussia generally .	Regulated in Berlin and some other large cities	7·4	100*	
London	Not regulated . . .	4·1	100	**Registrar-General, 1878, p.** xxii, average of 24 years.
Paris	Regulated for 100 years	26·3	100	**Bulletin Statistique, Municipal**, Ville de Paris, 1878. Published by the Prefect of the Seine.
Berlin	Regulated intermittingly for half a century	12·98	100*	
Scotland generally	Not regulated anywhere	9·2	100	Registrar-General for Scotland, 1876, p. xvi, average for 22 years, 1855-76.
Switzerland generally	Regulated only in Geneva	6·1	100	Official Report of Births and Deaths in Switzerland, 1879, average 26 years, 1851-76.
Edinburgh . . .	Not regulated . . .	9·0	100	Registrar-General of Scotland, 1876, p. 18.
Geneva	Regulated for many years	11·9	100	Official Reports for 1875-6, by the Secretary of State, J. F. Raymond.
Manchester . . .	Not regulated . . .	4·3	100	Registrar-General's Report, 1874-8.
Brussels	Regulated for 40 years	22·9	100	**Official Report for 1877, by** Dr. Janssens, Officer of **Health for Brussels.**
Liverpool . . .	Not regulated . . .	3·1	100	Registrar-General's Report, 1874-8.
Hamburg . . .	Regulated 100 years .	9·3	100	**Official Reports of Medical** Statistics for Hamburg, average of 9 years, 1871-9.
Denmark generally	Regulated in Copenhagen and 5 other cities	11·3	100	"Official Statistical Tabelwork," 4th series, Letter **A, No. 1, average of 5 years, 1870-5.**
Copenhagen . . .	Regulated for many years	22·5	100	**See above.**
London	Not regulated . . .	4·1	100	
Liverpool		3·1	100	

* Official Prussian Statistics from the Bureau of Berlin, 1880, p. 10.

French educational opinion on all such Acts.—At a conference held in London on 19th May, 1876, M. E. de Pressensé, who had been deputy for the department of the Seine, and had also served on a special commission of enquiry into the condition of the Hospital of St. Lazare, the chief "certified hospital" under the system at Paris, spoke as follows:—"I bring you the testimony of my personal conviction recently formed upon this great question. I have not entered into the details of your English legislation: I have not occupied myself with it, and I content myself with saying that I am convinced that it is not possible to stand still in such legislation. We must either go back or advance. It is a logical necessity which it is impossible to escape. . . . In our large cities a great trade in immortal beings is carried on. Who conducts it? That is the question. Wherever a system of sanitary laws concerning prostitution exists, as in France, I say it is the State which carries it on in the SOULS and bodies of human beings. It is the State which guarantees and patents it. I admit that it is not done with the object of directly encouraging immorality. I acknowledge that the question is a very complex one, and I cast stones at no one. There are many whom I respect who have fallen into the error which I combat, and I know that it is not in order to maintain immorality that they countenance this trade, but rather, as they think, to maintain public health and security. Let me say, first, that the fact of the State holding this market for debauchery is sufficient to destroy the idea of a State, whatever view of the State you may take. Well, I ask, can State represent law, can it represent justice, when it patents and recognises vice under its most hideous form? It is because a sentiment has been awakened of the true notion of a State that the State no longer farms out gaming tables, which were formerly established under its protection; shall not that which is true of gambling be held to be true of debauchery? . . . . The State (as in France) when it authorises and presides at a marriage, fulfils a high magisterial function. Well, I say, it does not seem to me possible that the State can recognise at the same time marriage and prostitution. Such a thing is an overturning of the notion of a State. I add, that the State ought to defend right and justice, but conformately to the law and giving protection to individual liberty. . . . You come to us with a measure which suppresses law for the pretended interests of the State. Let me ask whom it is you design to save? Certainly it is not the woman, who is the necessary victim of your system. No, you do not save the woman by your pitiless mechanism. You wish to save the young man. You wish to save his body. You speak not of his soul, and you are right. Do you save him in his body? *You do not?* Even here your system utterly fails. I cannot enter into details. I can but indicate the fact, and I say it is certain, even from the documents which have been furnished by the partisans of the system themselves, *that the evil which they desire to prevent increases every day.* For one victim that comes under your observation, there are thousands who escape you, and your measures of protection are useless. You cannot carry out your system, and up to the present time it has miserably failed. Now, could it be otherwise? You would regulate vice, but it is of the essence of vice to refuse to be regulated. Vice violates moral law, and you may expect it will transgress human rules. It is like a mighty river that has overflown its banks. It is a torrent whose fury you cannot arrest. You cannot say, 'Thus far shalt thou go and no farther.' It mocks at all your regulations. In my native city, which I have the honour to represent, it is certain that, since these regulations have been in existence, vice has increased in a truly frightful proportion. Do you know what you are doing? *You facilitate the first steps to vice. You make young men all over the country believe that debauchery at a certain age is a natural law which the State recognises; and thus you make the State the tempter of the young men. In facilitating these first steps, you favour immorality; for the patented evil has its recognised place in human legislation.*" —Amos.

Abolition in Paris.—Five years after this speech was delivered, the system was done away with in Paris. After a trial of nearly one hundred years, the Municipal Council of Paris suspected that this system was not doing what it proposed, and after due deliberation, came to the following decision:—"The Municipal Council, considering that the Institution of the 'Police of Morals' is destructive of the principles of individual liberty, *without succeeding in accomplishing the end it professes to aim at, either in the diminution of venereal disease*, or in the prevention of offences against public order and decency, resolves—That from and after the 1st January, 1881, the 'Police des Mœurs' shall be abolished, and that the following systems be established instead, viz.:—That free dispensaries be established for venereal patients, and that in all that concerns public order and decency, the ordinary police shall be employed, and that all offences against public order and decency shall be tried by the ordinary courts of law."

Zurich, Switzerland.—This city, after long experience of regulated brothels, abolished them in June, 1874, and an attempt to reintroduce them in 1880 was withdrawn by their proposers, rather than submit them to the vote of the Canton, which had indicated that it would not allow them again to be imposed upon the city.

Cape of Good Hope.—The Colonial legislature was induced in 1868 to pass a law based upon the provisions of the

English Contagious Diseases Acts. In 1869 a Select Committee was appointed to inquire into the working of the new law, and it was found that the medical men did not agree as to what was syphilis—that persons diseased from natural causes were placed upon the register as diseased from venereal causes; that women reported to be cured by one doctor were sent back to hospital for treatment by another; that the tickets issued to the women were looked upon as certificates to practise immorality; and *that the number of patients increased the longer the Act was in force.* Mr. Solomon, one of the members of the House of Assembly at Cape Town, introduced a Bill to repeal the Act, and after a hard struggle, in opposition to the then Government of the Colony, the Repeal Bill became law in 1872.

"The reference to Hong Kong has nothing to do with the Acts in England, as nothing in the shape of a license is issued in the protected districts." I suppose the people in those districts are quite aware that when the women are allowed to go free and continue their calling they are guaranteed by Government. And what a false guarantee it is, may be judged from the fact that the Acts have left the prostitutes in the protected districts in a far more dangerous condition than when they found them, the ratio of disease now being about 24 per cent higher than in 1866. It clearly follows that if the soldiers consort with the women in the manner explained by Dr. Barr, that disease must continue to increase.

Government makes an income from the houses at Hong Kong; it takes four dollars a month during the continuance of such licence. I do not know the total—perhaps fifty thousand dollars a year.

The report of Hong Kong Commission states, regarding the brothels licensed for the use of Europeans only—"These houses are resorted to principally by soldiers and the seafaring population passing through the Colony. . . . And in most cases, when men complain of infection, the disease appears to have been contracted in those brothels licensed for foreigners (Europeans) only, and subject to regular medical examination." (Rep., pp. 6, 7, and *Ans.* 14, 69-72, Summing up of Report). "Licensed brothels for foreigners are in themselves sources of infection, and the evidence before us POINTS TO THOSE ESTABLISHMENTS RATHER THAN TO UNLICENSED HOUSES, as the cause of disease to soldiers and sailors." The Commission go on to say that their conclusion *being on all points unfavourable to the Acts,* they *would have recommended their repeal,* and leave prostitutes to be dealt with solely as a matter of law and order, but that as the high naval and military authorities differ from them in their conclusions, drawn from the same evidence, they coincide with the authorities."

The same report, referring to the Colonial Surgeon's Department, says—"We feel bound to point out that those portions of the *Annual Medical Reports* which refer to the subject of the Lock Hospital *have, in too many instances, been altogether misleading.*" Page 2, parag. 2.

"In 1867, after ten years' experience, the PUBLIC was informed that the ordinance had been on trial for nearly ten years, and *had done singular service.*" Yet in this very same year, 1867, 19th April, Dr. Murray stated in an official report, NOT INTENDED FOR PUBLICATION, "that venereal disease has been *on the increase*, in spite of all that has been done to check it, *is no new discovery;* it has already been brought before the notice of his Excellency"! (Rep., p. 35, parag. 4 and 5.)

This same Dr. Murray's Annual Report *for the public* for 1867 was put in evidence before the Lords' Committee on Venereal Diseases, 1868 (p. 135). "Venereal disease has now become of *comparatively rare occurrence.*" Yet the Army Report for the previous year (1866, p. 115) states that the admissions to hospital for venereal diseases were 281 per 1,000 men—*i.e.*, more than one man in four of the whole soldiery was in hospital for this "COMPARATIVELY RARE" disease!!

As to the 5th Fusiliers and Dover having been an unprotected station in 1868, I am obliged to Mr. Lowndes for the correction. The matter was taken from a non-professional authority, and I have caused a printed slip, indicating the error, to be sent to every medical man to whom the pamphlet was addressed. As Mr. Lowndes says, I am not "familiar with the facts and the progress the investigation of the subject has made within the last few years," and as this paper is an evidence of that, Mr. Lowndes will, I am sure, pardon me.

In referring to the expenses of the working of the Acts, Mr. Lowndes says, "the deterioration of constitution *avoided by reduction in the frequency of disease is an important element.*" Where is the proof of reduction in any constitutional form of disease? The evidence by Inspector-General Lawson clearly proves that the soldier would be safer in the unsubjected districts:—Syphilis has increased, gonorrhœa has not been reduced, and the ratio of diseased women is 24 per cent higher than in 1866. The only reduction—a worthless one altogether—is in soft sores, and this is not in greater proportion than that in which it was taking place before the Acts. Who is being protected in the protected districts? Neither the soldiers nor the prostitutes!

As to benefits derived by the navy, no evidence was given before Committee about the navy; and I am inclined to think it would have looked still worse for the Acts. It was better withheld for the Acts' sake.

Petitions.—There were presented to the House, in favour of repeal of the Acts, during the years 1870 to 1881 inclusive 10,315 petitions, with 2,105,503 signatures. Total number of petitions to Parliament against repeal of Acts for same period, 45. Total signatures, 3,579.

In 1873 there was only 1 petition against repeal, and that was signed by 4 persons! There has not been a single petition in favour of the Acts since 1873.

In 1872, of 34 petitions against repeal, with 3,366 signatures, 28 petitions, with 2,983 signatures, were from places subjected to the Acts; and of these signatures, 1,112 were those of women "brought under the influences of the Contagious Diseases Acts."

Cromwell and the Contagious Diseases Acts.—Mr. Lowndes says, "And I think that the opinion of Cromwell as to these Acts would most probably have been the reverse of Dr. Patterson's. While exhorting his soldiers to be virtuous, he would, I think, have preferred to locate them in protected districts." A more wildly erroneous opinion Mr. Lowndes has never given. On the 10th May, 1650, an Act was passed, part of which is to the following purport:—"In case of fornication *both* parties were to be punished with three months' imprisonment, without bail, and were to give security for good behaviour for one year. Every common bawd, be it *man* or *woman*, wittingly keeping a brothel, for the first offence was to be openly whipped, set in the pillory, branded with the letter B, &c." Rude, stern justice; but still justice, as between the sexes. True, Cromwell is said to have told his soldiers to "Trust in God and keep your powder dry," but he certainly did not believe that providing a "supply" of prostitutes, and keeping them clean for his men, was the right way to that. He says, "I raised such men as had the fear of God before them, as made some conscience of what they did; and from that day forward, I must say, they were never beaten, and wherever they were engaged against the enemy they beat continually." These were the Ironsides—*Carlyle*, vol. iii, p. 250. Again, "Not a man swears but he pays his twelve pence—no plundering, no drinking, disorder, or impiety allowed!" "They were men that had the fear of God, and gradually lost all other fear."—*Carlyle*, vol. i, p. 133. I am no advocate of war or fighting, nor am I an apologist for many things recorded of Cromwell, but as the question in connection with the Acts is, that they were passed "for the greater efficiency of the army and navy," I trust that I may be pardoned for giving the opinions of one who was considered a master in his way. The opinion as to Cromwell's hypocrisy is widely entertained; however that may be, no one has ventured to say the same of the author of *Paradise Lost;* and on the field of battle those soldiers were found to be "conclusively genuine." I give another name, of which we all are proud—John Bunyan; he was a private in the ranks, and no doubt one who could "lay about him," and he certainly did not deem vice a necessity.

State Supported Locks or Hospitals for the Propagation of Vice.—Mr. Lowndes refers to our Lock Hospital, and the fact that the managers had withdrawn the sum of £180 from the bank in 1881. Of course, and what of that? I suppose most of us, who are fortunate enough to have anything in the bank, draw when we require to do so. I may state that, when money was asked for the extension of the hospital in 1869, prior to the Police Act, the sum of £3,659 was collected in a few days. Mr. Lowndes may accept it from me that we are on the safe side of bankruptcy, for some years at all events. With us here there are no advertisements or canvassers ever employed. Our excellent superintendent, Mr. Condra, himself a thorough man of business, collects the annual subscriptions personally, whilst Mrs. Condra, who as a matron has no superior, sees that nothing is wasted. They have both held their responsible office for the long period of twenty-seven years. The people of these islands have an instinctive aversion to support vice, and long may they retain it; but there are Christian gentlemen who never object to assist for the purpose of curing disease, however contracted. This is a very different affair from being taxed against your will for the propagation of vice.

The effect of such hospitals, in my opinion, would be simply ruinous to the morality and best interests of our country, to the chastity of our females, to the happiness of our homes. No one, of course, would for a moment say that they were intended to promote vice, but incontestibly that is what such Acts must do, wherever planted, because whatever is supposed (and it is only supposed, because vice can never be rendered safe), to render vice safe, will increase its practice, and whatever increases prostitution will inevitably increase venereal disease. Syphilis cannot at all be viewed in the same light as other infectious diseases, which very properly come under sanitary regulations. With all our Contagious Diseases Acts and Government hospitals, we are falling into the fatal error of trying to cure a disease by intensifying its cause. Venereal disease may be viewed, Abernethy would say, as "the local manifestation of a constitutional disease," and the disease here is a moral one, and I much fear if any other than moral means will check it. Then, if such Acts and hospitals tend to increase immorality (*vide* table of illegitimacy), how is it possible by them to reduce disease?

"There can be no ground whatever for the enormous machinery proposed to be called into existence for the cure of venereal diseases as contra-distinguished from all other complaints, except that every one must acquiesce in the permanence of male vice and abet it. Free public hospitals in every town for none but venereal diseases, mean that the ratepayers or taxpayers, as a class, have a greater concern in women being safe for promiscuous intercourse, than they have in the same women being cured of every other complaint that flesh is heir to but a venereal one."—*Amos.* The inevitable result of such Acts, if carried out, leads to the inexorable conclusion that our people would be reduced to Atheism; and in the long run, a nation of the bravest men and most beautiful women on the face of the earth, be brought to a level with the beasts of the field.

Remove the cause, and the effect will cease.

[MY DEAR DR. COATS,—I have to apologise to you and the many readers of the *Journal* for the contents of this paper. Details are necessarily indelicate. The matter has been forced upon me, and I have now finished.—A. P.]

CASE OF ACUTE FŒTID EMPYEMA, TREATED BY INCISION INTO PLEURAL CAVITY, WITH COPIOUS ABLUTION; RAPID RECOVERY.

BY GEORGE BUCHANAN,
Professor of Clinical Surgery in the University of Glasgow.

THE following case presents many points of interest—medical, pathological, and surgical—but as the latter aspect seems the most striking, I have been asked to act as reporter. The patient was under the care of Dr. Whitson, but as she was a family connection of his, he from the outset asked Dr. Maclaren to direct the medical treatment. The case proving very serious, Professor Gairdner was asked to co-operate as consultant, so that all the features, some of which are very unusual, if not unique, can be vouched for by competent observers.

Miss D., aged 19, enjoyed good health till the 24th September last, when she began to suffer from pains in the left side of the chest. On the 28th the symptoms became more severe. She had pains over the left mammary region, increased on taking a deep respiration, and on applying the stethoscope over the painful part a friction sound could be heard. There was dulness over the left lung behind, and diminished respiratory murmur. She had a short cough, but no expectoration, and she lay on her back well supported with pillows, as the pain was worse when she attempted to lie on her left side. Her temperature rose to about 101° and her pulse to 130. The treatment at this stage consisted in moving her bowels well, keeping poultices constantly applied to the painful part, and giving her a diuretic mixture containing iodide of potassium. The symptoms became gradually more severe. Effusion into the left pleura was evidenced by dulness on percussion over the whole posterior region, and anteriorly as high as the third rib, and by displacement of the heart to the right of its normal situation. Her respiration rose to 36 per min. and her pulse to about 140. On 3rd October Dr. Gairdner saw her for the first time in consultation. He thought her illness a serious one, but had hopes that in a short time the acute symptoms would abate under the treatment pursued. He saw her again on the 7th and 12th October, and on the 14th all hopes of being able to cope with the disease without recourse to thoracentesis were given up, as she was then in a dangerous state owing to the prolonged high fever, the greatly enfeebled and very rapid heart's action, and the great and apparently increasing dyspnœa.

On the 14th October I was called on to perform paracentesis thoracis with the aspirator in the following circumstances. Miss D.'s pulse was 140; respirations, 50; countenance anxious; breathing oppressed; could not lie down owing to the sense of suffocation; left side of chest bulged, but not decidedly at the intercostal spaces; respiratory movement of left side impeded; percussion dull all over left side; heart displaced toward middle line; no pneumothorax or any evidence of connection between the pleural cavity and bronchial tubes.

I made the puncture two inches below the point of the scapula between the seventh and eighth ribs, and drew off about 70 ounces of dark brown putrid pus of most offensive odour.

The operation was attended with most gratifying results, the patient having slept during the following night several hours at a time, which she had not done since the onset of the disease.

The fluid rapidly re-accumulated, so that on the 19th matters were much as described on the 14th. The operation was therefore repeated, and 20 ounces of pus removed. In this case the pus, though still putrid, was not so dark in colour and more watery.

In both instances the pus was submitted to microscopic examination and the presence of bacteria established, but this was forty-eight hours after evacuation, so that there is no proof of their existence in the pus while it was in the pleural cavity.

The relief after the second tapping was as apparent as before; but again, in twenty-four hours, the fluid was evidently accumulating. Accordingly, with the concurrence of the other medical attendants, I gave the patient chloroform, and made a free incision into the pleural cavity, near the site of the puncture. I introduced my forefinger into the chest to explore the cavity, and found the pleura costalis covered with a soft pulpy membrane, the upper surface of the diaphragm was smooth, but I could not reach the pericardium with my finger. The lower edge of the lung felt soft, but was so far off that I could only touch it. I now introduced the two tubes of a Gooche's double canula, crossing them like the letter X, and washed out the pleural cavity with tepid water containing Condy's fluid, using an india rubber tube as a syphon. About eight gallons of fluid were made to flow through the chest till it ran out perfectly pure and odourless, and of the original colour of the Condy's fluid as diluted. A large vulcanite tracheotomy tube was passed into the chest through the opening and secured there to ensure drainage.

The effect of this apparently rude proceeding was marvellous; most marked improvement in breathing, pulse, and temperature, and general comfort. Patient slept some in the afternoon and several hours during night. Occasionally she was troubled with a tickling cough, for which camphor dissolved in chloroform was tried. There never was much expectoration; and that of clear mucus.

From this time onward the progress to recovery was uninterrupted and rapid. Pus in very small quantity and of no offensive odour continued to be discharged for some days by the tube; but by the 11th November it had become blocked up, so I took it out, giving exit to a little healthy yellow pus. I again washed out the chest with water and Condy's fluid; but it ran clear at the very first. Instead of the hard vulcanite tube I put in a soft ordinary drainage tube, about 6 inches long, and secured it in its place.

Through this a few drops of pus were discharged daily, but this completely dried up before the tube was finally removed on the 22nd November. In a few days the opening into the thoracic wall was completely closed—that is, in about five weeks after the incision—after this the patient rapidly regained health and strength.

Remarks by Professor Gairdner.—This case is unique within my experience, in respect of the rapid formation of an acute empyema, not only dangerous and extreme in its symptoms from the first, but in all probability septic and even gangrenous, apart from any primary lesion of the lung or other organ or part, such as in the great majority of cases determines a really fœtid empyema. Even in a somewhat extended pathological experience, and among many hundreds of observations in my own cases and those of others, I cannot remember to have ever witnessed a positively putrid collection of this kind, in which there was not at the same time either fœtid abscess or gangrene of the lung, or, on the other hand, a perforation leading to pyo-pneumothorax, with septic contamination of the effusion; and even in cases in which leakage had taken place from the pleura into the lung through a superficial slough of the former, the absence of distinct septic contamination has usually been rather remarkable. Moreover, the symptoms in this case approximated closely to those of the rare and dangerous form designated by Fræntzel as "pleuritis acutissima," in which a fatal result is almost unavoidable, whether or not evacuation of the contents of the pleura is practised.* Such cases, apart from complications, are undoubtedly exceptional. In the first twenty years of my experience I can recall only one, and perhaps one or two at a later date. "Such cases are rare," writes Dr. Clifford Allbutt in probably the latest English résumé of the subject, "except as complications of septic and other diseases, and they are almost surely fatal, even after free evacuation of pus by incision."† When, therefore, in the present case, after twenty days of accumulation, the aspirator gave vent to a pus so horribly fœtid that the first gush of it was almost intolerable, even in a large airy apartment, the mind was led irresistibly to the idea of some latent primary gangrene, either in the lung itself, or in some other viscus with secondary gangrenous abscesses forming in the lung. A certain amount of equivocal odour, suggestive of possible septicæmia, had indeed been detected in the breath and transpiration of the skin, and had formed one of the elements of a grave prognosis; but, on the other hand, there had never been any but the most insignificant expectoration: and even after the first aspiration of the chest, most careful observation failed to detect any evidence of pulmonary lesion on the one hand, or of pneumothorax on the other. After the second aspiration it became only too clear that nothing could possibly save the patient except free incision, and washing out the cavity; but we hardly ventured to hope that these measures would be so rapidly successful, and that the source, whatever it was, of septic decomposition would be not only reached but apparently removed by one, or at most two, ablutions of the cavity with diluted Condy's liquor. The result, unexpected and gratifying as it was, deserves to be recorded, even although it leaves the question of the source of septic infection as obscure as ever.

---

* Ziemssen's *Cyclop. of the Practice of Medicine.* Vol. iv, p. 602.

† Quain's *Dictionary of Medicine*, p. 1213. *Note.*

CURRENT TOPICS.

Glasgow and West of Scotland Medical Association.—*Editors' Report.*—The following report was presented to the Annual General Meeting, held in the Faculty Hall, Glasgow, on the 30th January:—

The past year has been signalised by several changes in the arrangements of the *Journal.* The most important of these was the appointment of Dr. Alex. Napier as co-editor. Dr. Napier has for years occupied the office of sub-editor, superintending chiefly the important department of "Medical Items." He has, however, during the last 18 months, taken a considerable amount of the regular editorial work, as the editor was engaged preparing for the press a somewhat comprehensive work on Pathology. In the month of July Dr. Coats proposed that Dr. Napier should be asked to assume the position of editor, and this the Committee readily agreed to.

The other two changes in the *Journal* have involved a considerable increase of expenditure, and it was because the finances of the *Journal* seemed to be in a sufficiently prosperous condition that they were proposed. The first of these was the employment of a better paper on which to print the *Journal*, so that it might have a more substantial appearance. The other was the presentation, to each author of an original article, of 25 gratis copies of his article. It was hoped that this, forming some recognition of the services of contributors, would be an incentive to some to send their papers to this *Journal*. The arrangement was only adopted, however provisionally, and its continuance may depend on whether the Committee think it has served the object aimed at, and whether the financial position of the *Journal* will allow of it.

Looking to the financial position of the *Journal* and its prosperity generally, it is exceedingly desirable that the number of subscribers should be increased. Every year a certain number of subscribers lapse, and it is necessary to fill their places by new ones. This has been effected hitherto chiefly through the issue of a circular at the beginning of the year asking for new subscribers, and the result has been that the *Journal* has fully maintained its position. This circular has not been issued this year, and the editors appeal to the subscribers generally to endeavour to obtain new names. Looking back on the contributions for the year, the editors feel some confidence in appealing to the subscribers, as they believe that at no time has the *Journal* reached a higher standard, both as regards the excellence and the varied character of the papers.

The editors would again direct attention to the department of the *Journal* set apart for the publication of *Hospital Reports*, and would urge on Hospital Physicians and Surgeons the duty of continuing and even augmenting the support they have given to it in the past. It is a department which should show better than any other the amount and kind of work being done in our hospitals; and with the immense stores of material at hand it should not be difficult to have it always well represented.

PROGRAMME OF A PRIZE ON THE PREVENTION OF BLINDNESS.—The fifth international Congress of Hygiene, which will meet at The Hague, Holland, in 1884, will award the prize of two thousands francs (£80 sterling), offered by the London Society for the Prevention of Blindness, to the author of the best essay written in English, French, German, or Italian, on "The Causes of Blindness, and the Practical Means for Preventing it." Besides this prize, the International Society for the Improvement of the Condition of the Blind reserves to itself the right to award a second prize of one thousand francs (£40 sterling), or two prizes of five hundred francs (£20 sterling) each and a silver gilt medal with a diploma, should it see fit, to such of the essays as should, in the opinion of the international jury for the principal prize, be deserving of it; the last mentioned prizes will be distributed at the centenary festival of the first blind institution founded by Haüy, which will take place in Paris in 1884.

The fourth International Congress of Hygiene, which met at Geneva in September, 1882, has adopted for this competition the following programme, as prepared by the London Society for the Prevention of Blindness:—

I. *The Study of the Causes of Blindness.*—*a.* Hereditary causes. Diseases of parents, consanguineous intermarriages. *b.* Infantile eye diseases. Various inflammations of the eyes. *c.* School period and time of apprenticeship, progressive short-sightedness, &c. *d.* General diseases. Diatheses, various fevers. Chronic poisoning, &c. *e.* Trade influences. Wounds and accidents, &c. Sympathetic ophthalmia. *f.* Social and climatic influences. Contagious ophthalmias. Unhealthy habitations, defective lighting, &c. *g.* Neglect of treatment and bad treatment of eye affections.

II. *The Study of Practical Preventive Means.* *a.* Legislative means. *b.* Hygienic and professional means. *c.* Educational means. *d.* Medical and philanthropic means.

The international jury elected by the Geneva Congress for the purpose of judging the essays, consists of:—HOLLAND, Dr. Snellen, professor of ophthalmology, Utrecht. GERMANY, Dr. Varrentrapp, Frankfort; Dr. H. Cohn, professor of ophthalmology, Breslau. FRANCE, Dr. Fieuzal, physician to the Hospice des Quinze-Vingts, Paris; Dr. Layet, professor of hygiene, Bordeaux. ITALY, Dr. Reymond, professor of ophthalmology, Turin; Dr. Sormani, professor of hygiene, Pavia. ENGLAND, Mr. Streatfield, professor of ophthalmology, University College, London; Dr. Roth, honorary Secretary and Treasurer (*pro tem.*) of the Society for the Prevention of Blindness, London. SWITZERLAND, Dr. Dufour of the Ophthalmic Hospital, Lausanne; Dr. Appia, Geneva; Dr. Haltenhoff, lecturer on ophthalmology, Geneva, and Secretary to the Jury. Dr. Appia and Dr. Varrentrapp having resigned, the jury completed its number by electing Dr. Coursserant, oculist, Paris, and Dr. Berlin, professor of ophthalmology, Stuttgart.

Those essays to which prizes have been awarded will become the property of the Society for the Prevention of Blindness, and of the International Society for the Amelioration of the Condition of the Blind, who will be at liberty to publish them in whole or in part in several languages, in order to make them useful in the way they consider best.

The (inedited) manuscripts for competition are to be sent to the undersigned Secretary not later than the 31st March, 1884. Every manuscript has to be distinguished by a motto, which is also to be written on a sealed envelope containing the name, Christian name, titles, and address of the author. The envelopes will not be opened until after the award of the jury.

DR. HALTENHOFF, *Secretary to the Jury.*

Obituary.

JAMES G. LYON, M.D.

WITHIN recent years death has been busy among the younger members of the profession in Glasgow, and this month we have to add yet another name to this sad record of mortality. Dr. J. G. Lyon died at his residence, 276 Bath Crescent, on the 6th of January, in the forty-third year of his age.

James George Lyon, the last representative of a very ancient, and in some respects, notable House, was born at Forfar in 1840; but the family soon after removed to Stirling, where he spent his boyhood, and received his preliminary education. At an early age he entered the Arts' classes at Glasgow University, and passed to the degree of M.A. in 1861. He afterwards enrolled himself in the Medical Faculty, and took his degree of M.D. in 1864. There was one distinction during his academic career to which he looked back with peculiar satisfaction—the receiving the prize for clinical surgery at the hands of such a master as Lister, for whom, through life, he had the most discriminating admiration, and whose practice he carried out with the minutest details.

After his graduation he acted for some time as assistant in England, and finally settled in Glasgow.

At an early period of his career he evinced a strong bias towards the surgical aspect of his profession, and shortly after the opening of the Western Infirmary he was appointed one of the surgeons to the dispensary. The work he did in this capacity, and also while on temporary duty in the wards, was of the most able, painstaking, and judicious kind; yet it met with no recognition at the hands of the Directors, who, on the election, about two years ago, of an additional surgeon to the hospital, set aside his many, and as some thought, paramount claims, and appointed an outsider. This rejection made a deep impression upon him, but he was too self-reliant to make much complaint; and even his intimate friends rarely heard him allude to it. His health was never robust, and he had been for many years the subject of a chronic cough. About twenty months ago his strength began to fail somewhat, and his temperature showed a tendency to an evening rise. Still he was very unwilling to go out of harness, and worked on up to the end of January, 1882. It was then quite evident that the apex of his right lung was seriously involved, and he himself became alive to the fact that his hereditary enemy was upon him. A three months' residence in Guernsey and a voyage to the Cape in no way arrested the progress of his disease, and for the last three months of his life he was closely confined to bed. During his illness he was calm, resigned, and even cheerful—a brave heart "taking the measure of an unmade grave," yet bearing up for the sake of others. His unselfishness, indeed, came out more strongly as his own bodily infirmities increased, and among his last articulate words were those of comfort and consideration for the sorrowing friends who stood around his bed.

Dr. Lyon had all the qualities which go to make an eminently successful surgeon, and his merits were gradually asserting themselves among his fellow-practitioners, many of whom had come to place no little reliance on his opinion and skill. He was slowly, but surely, falling into a good consulting practice. As an operator he was very successful, and had a wonderful facility for adapting himself to any emergencies which might arise. His results in cases of cleft palate, genu valgum, excision of the elbow, &c., were such as any surgeon might be proud of; but they were the outcome of the most careful operative procedure and patient after-treatment. He had nothing at all of the so-called "dash" about him. He did his work for the work's sake, and many of the cases which involved the greatest anxiety and care were those in which he well knew he could expect no money recompense. By his patients he was held in high esteem; looked upon as a kind and considerate friend.

He did not contribute much to the literature of his profession, but his paper on the adaptation of his axis-traction rods to the midwifery forceps, is well worthy of observation, and he himself had no little merit as an accoucheur.

By his personal friends he was warmly beloved for his singularly transparent and upright character. He was the soul of integrity. Nothing that savoured of "trimming," far less of questionable dealing, had any place in his nature, and no matter how strongly interest might point against such a procedure, he denounced these practices and their advocates in the most emphatic terms. He had also a kind and gentle heart, and his deeds of mercy were many and unostentatious. His death has occasioned a serious loss to the profession in Glasgow, and for his personal friends not a little of the "light of life" has gone out.

In addition to his M.D. he held the diploma of F.R.C.S.E., and he was for some years before his death a certifying factory surgeon for Glasgow.

REVIEWS.

Lehrbuch der Kinderkrankheiten, für Aerzte und Studirende. Von Dr. Adolf Baginsky. Braunschweig: Friedrich Wreden. 1883.

This forms the sixth volume of the series of medical handbooks now being published by Wreden of Brunswick, and may be described as a creditable addition to the literature of a special branch of the profession, which has not, as yet, been over written. It is designed, as its title indicates, for practitioners as well as for students, and the general plan upon which it is constructed seems to be to give a condensed epitome of the whole subject as it stands at the present day, rather than, as is often the case, to give special prominence to certain points, upon which an author concentrates his energies to the sacrifice of the subject generally. Another danger which specialist authors run, but which we must admit Dr. Baginsky has succeeded in avoiding, is to magnify the importance of his specialty, and overlook the immense importance of never for a moment losing sight of the relation which a branch of a science bears to the stem, or a mere portion to the concrete unity of medical science. That the author has clearly set this before him as a part of his task is obvious from the following sentences, which we extract from his sensible preface. Speaking of the recent advances which have been made in the knowledge of his subject, he says:—

"Earnest work, involving anatomical and physiological research, has disclosed an organisation in the child which differs essentially from that of the adult, and which only develops itself by degrees upon the latter model. But it is exactly such special work which places the diseases of children upon a common basis with the sciences of ordinary medicine, of physiology, and of pathological physiology. The results which have been attained have been a gain to general medical science, and are of all the greater importance, because they conduce to the solution of problems in general medicine, by means of original research in an independent organism, which is at the same time undergoing development. I may refer, in illustration, to the conclusions which have been reached with reference to growth, nutrition, interchange of chemical elements, the development and secretion of the glands, the development and function of the nervous system.

"Differing, as it does, anatomically and physiologically, from the adult, the child reacts in a different manner upon the causes of pathological phenomena. This gives to the diseases of childhood a peculiar character and course. The pathological changes, the course of fevers, the complications, and the reactions of the nervous apparatus, have all, in part, a special character of their own. Therein lies the necessity, scientific as well as practical, for treating the healing art in children as a special department; and this all the more, seeing that certain drugs and therapeutic measures act differently upon the child and upon the adult."

Acting upon these convictions, the author divides his work into two parts—General and Special. Under the former he considers the special physiological conditions of childhood, and his observations on this head include a brief, but sufficient notice of the circulatory, respiratory, and digestive apparatuses, of dentition, of the urinary apparatus, the nervous system and organs of special sense, the skin, temperature, &c. Passing, then, in review, the growth of the child and the principle which should guide us in advising as to its nutrition, he proceeds to give full directions as to the special methods which should be adopted in investigating disease in the child. After calling attention to the special difficulties which prevent a confident diagnosis in these cases, he indicates the various points, from the observation of which we may glean information by adopting a methodical and exhaustive method of investigation. He shows, first, that the attitude of the child is so far significant, and that while the healthy child instinctively maintains the posture which it had while in the womb, in the first month after birth more particularly, there are certain affections, such as stiff neck, earache, respiratory embarassment, &c., in which the head is raised and bent backwards. Orthopnœa, he adds, is rare in children, whereas a prone position, although not always explicable, is of more frequent occurrence. The colour of the skin, the expression of the face, the nature of the perspiration and pulse, the tone of the cry, and the sound of the cough, are all mentioned in detail as sources of possible information. The author then, following the same plan, proceeds to the explanation of the various regions, treating successively of the head, throat, thorax, and abdominal organs, and the manner in which each region is to be best explored. This section of the work terminates with some brief general observations on the subject of ætiology and therapeutics.

The Special Part occupies the bulk of the book, and in reference to it, it is only possible to give here a very rapid analysis of the method which Dr. Baginsky has followed in passing under review the various branches of his subject. Beginning with a sketch of the diseases of the new-born child, in which the rarer affections of this period are not overlooked, he discusses the acute infectious diseases of childhood, including the exanthemata, cerebro-spinal meningitis, diphtheria, cholera, intermittent fever, &c. Under chronic general diseases, are included anæmia, chlorosis, leucæmia and the hæmorrhagic diathesis, scrofula, syphilis, diabetes, &c. To the diseases of the nervous system nearly 140 pages are devoted, and this most important class of cases is treated with marked ability both as regards classification and diagnosis, while the basis of sound treatment is in most instances satisfactorily indicated.

The diseases of the respiratory, circulatory, and digestive organs are then successively treated, and under each heading there are few points known to medical science which escape the notice of our author. One or two brief extracts will give a fair idea of the manner in which he manages his subjects. For example, in regard to the symptoms of bronchial asthma, he writes:—

"The attacks commence, sometimes in connection with existing bronchial catarrh, and sometimes independently of this. Without fever, and without any marked changes which can be physically detected in the thorax, there is developed under the eye of the physician a mode of respiration which steadily become more difficult. The breath is drawn in in short, hissing, wheezing inspirations; the expiration is blowing and often accompanied by sighing. The face is pale, the eye deep, the nose pointed and cold, and the extremities cold. The pulse is imperceptible or very feeble, and the tension of the radials slight. Careful physical examination shows a marked depression of the diaphragm, the lungs are abnormally distended. Percussion is clear, almost tympanitic; auscultation reveals only wheezing and humming râles. Gradually, the dyspnœa begins to relax, the respiration becomes easier, deeper, more free. Slight coughing fits bring up in older children a scanty, clear, or finely frothed sputum. Leyden pointed out in them the presence of peculiar sharp crystals in the adult, which are perhaps to be found in the child also, but as yet they have not been looked for. Sometimes exhaustion and sleep come on, out of which the child awakes quite cheerful and well. The recurrence of the attack varies greatly, sometimes daily, sometimes at intervals of several days, and in general they cease gradually and a cure is the result."

Speaking of exophthalmic goître, he points out that, while this disease is comparatively rare in infancy and childhood, it is much more common at all ages in the female sex. After a short abstract of the cases recorded as occurring in early life, since the disease was described by Basedow in 1840, he gives a short account of a case from his own experience which we may here quote in an abridged form, more particularly as it gives an idea of the chief modes of treatment which are adopted in this singular affection at the present day.

"The case to which I refer is that of a girl of 12, of very fair complexion and well developed, who had often been ill treated by a drunken father and a cruel mother during the night. This intelligent child gave unhesitatingly as the cause of her illness the frights to which she had been subjected. She manifested restless movements, but not marked chorea; large white stains (*vitiligo*) over the whole skin, prominent eyes, of medium size, and unequal pupils were also noticed. The middle portion and right lobe of the thyroid gland were considerably enlarged. Over the whole gland one could feel a continuous thrill, in connection with which a prolonged systolic murmur was audible. There was pretty strong pulsation in the temporal arteries. There was extensive and very marked palpitation, with enlargement of the left ventricle. The heart sounds were normal. The child was treated by the application of the induction current to the sympathetic in the neck, along with the external application of the unguent. potas. iodide over the enlarged thyroid; internally, by the administration of ergotine, and afterwards of digitalis, and by these means she was completely restored. Even the white stain of vitiligo disappeared almost entirely."

In noticing the occasional occurrence of broad condylomata in the tongue and the mucous membrane of the mouth, under the head of Stomatitis Syphilitica, he says:—

"These manifest themselves in the form of whitish-gray, well marked, roundish infiltrations, extending over the mucous surface, and distinctly raised above the surrounding tissues, which are for the most part pinkish, or of a deeper red colour. In this form they are easily recognised, but it must be admitted that a confident diagnosis is only possible when other symptoms of syphilis simultaneously exist, such as the exanthemic form, coryza, syphilitic laryngitis, &c."

An outline, as complete as the circumstances admit, of the diseases of the urino-genital apparatus, forms the subject of the next chapter, and the book ends with a study of the affections of the organs of special sense to which children are subject, and, lastly, with the diseases of the skin and appendages. It would be too much to call the book an exhaustive treatise of the subject of which it treats, but we do not hesitate to repeat the conviction which we have already expressed, that we have here a creditable and useful contribution to professional literature, and one which, by the general practitioner, as well as by the specialist, is likely to be highly prized.

Human Morphology: A Treatise on Practical and Applied Anatomy. By HENRY ALBERT REEVES, F.R.C.S.Ed. Vol. I. The Limbs and the Perinæum. London: Smith, Elder, & Co. 1882.

WE must confess to having entered on the perusal of this work with not a little prejudice against it, based mainly on the fact, so apparent at the first glance, that a large proportion of the very numerous woodcuts illustrating it are borrowed from standard works on anatomy without any acknowledgment whatsoever. The author endeavours to disarm criticism by a statement in the preface that he has drawn upon certain well known foreign anatomical works, but adds that "a few of the illustrations are from English sources, and are acknowledged in the text." He does certainly acknowledge those taken from Marshall, Cunningham, Ewart, and Smith and

Walsham (not more than a dozen in all), but his indebtedness to Quain's *Anatomy* and the works of Struthers and John Wood is in no way referred to; and this is the more strange, seeing that to Quain's *Anatomy* alone he owes not less than twenty of the most effective woodcuts. Those plates which appear to be original are in many instances so atrociously bad so defective in drawing, and so devoid of anything approaching a due proportion in the relative size of arteries, nerves and muscles, that they go far to condemn the work. Another consideration leading also to an unfavourable estimate, is the large size of the book. Here is a volume of over 700 pages, yet in it the author only describes the dissection of the limbs and the perinæum. Two other volumes are to follow, which will certainly not be less in size than the present one, so that the student who takes this as his dissecting manual will have reason to bewail the brevity of the period of study, and will find the four years usually allotted to the curriculum all too short for the study of anatomy alone.

We mention these objections at the beginning of our notice because we are sure they will suggest themselves to every teacher of anatomy on first taking up the book, and because a more careful study of it has served materially to modify the first unfavourable impression. A more thorough acquaintance with the work convinces us that it is carefully, nay, even laboriously written; its general accuracy is beyond dispute, although there are no doubt occasional slips, such as will occur where such a multiplicity of details have to be described. These minor inaccuracies we are convinced Mr. Reeves will be glad to have pointed out to him, and will accept our assurance that they are mentioned in no carping spirit, but with an honest endeavour to assist in what is doubtless his main aim, namely, the furtherance of sound, thorough, and exact anatomical teaching.

The Introductory Observations might well have been omitted, as occupying space without giving any material instruction to the student; we must, however, make an exception in favour of the tables of measurements of the different parts of the body, for these so rarely figure in anatomical works that we are glad to have them here for ready reference. The statement (page 40) that the hand equals in length the distance from the occipital protuberance to the root of the nose is simply preposterous; if the line be carried (as we suppose Reeves intends) over the vertex, it measures ·37 metre, while the hand, according to Krause's table, measures ·196 metre, so that the line is nearly double the length indicated. The two first woodcuts are unfortunate in regard to their markings, for in Fig. 1, what is said to be the episternal notch is evidently the cricoid cartilage, and no one who accepts 15 as denoting the middle of the femur will admit that 14 indicates the junction of the middle and upper two-thirds of the same bone. In Fig. 2, in like manner, the markings 8 and 9 for the gluteal and ischiatic arteries cannot both be right, for these arteries are not situated on the same horizontal plane; No. 24, moreover, has no description attached, and it will puzzle others besides students to say what it is intended to point out the position of. The chapter on Anatomical Technics is a most valuable one, although, we fear, likely to be of little practical use, excepting to lecturers and demonstrators. Every anatomical teacher has experienced in his early days the difficulty of obtaining reliable information as to modes of preparation, and has had to search in the dusty literature of a byegone age for information which modern text books fail to give; the anatomical manuals of our continental neighbours (especially the French) are not so defective in this respect, and we think Mr. Reeves has done well to follow the example they have set. The methods described are generally reliable, and are very varied, but the "paint injection," which in recent years has given such excellent results, is conspicuous by its absence. In speaking of the water bath for heating the subject, he refers to a hot bath of the temperature of 33°; possibly he means degrees Centigrade, but, wanting this information, we fear the preparer would find the heating process a slow one. The description of the mode of making frozen sections has been written by Dr. J. G. Garson, by whom the frozen sections in the Museum of the College of Surgeons of England were made; the account given is clear, and minutely accurate. We may note, in passing, that some of the earliest specimens obtained in this way in this country, were those prepared at the Glasgow Royal Infirmary School of Medicine, by Dr. Symington and Mr. Clark, in the winter of 1879-80.

When Mr. Reeves speaks of the "early half of the day" being usually allowed for the dissection of the perinæum, one is led to doubt if he has had any practical experience of dissecting room work; but we are a little reassured by finding him on a later page speaking of the dissection being completed in "the first day or two." We agree with Cunningham in thinking the two days usually allotted to the dissection far too short for the study of this important region, and are sure that no student could do justice to the forty pages in which our author describes it, in even the longest of the two periods mentioned by him.

The dissection of the upper limb, lower limb, and perinæum is described with great fulness, and with a minuteness and accuracy not attempted even in the elaborate *Demonstrations of Anatomy* of Ellis. There is, however, a great deal of needless and wearisome repetition, both in the letterpress and illustrations; the descriptions are often anything but clear, even to one who already knows the subject, and for the most part the book is anything but easy reading. As examples of the obscurity complained of, we may cite the following:—In speaking of abduction and adduction at the shoulder joint, the author says, "In both cases the humerus rolls down in the glenoid cavity in the first motion, and up in the latter;" here, clearly, the deletion of the first three words will make the sentence intelligible. Our second example is not so easily explained. It occurs in the description of the interosseous muscles of the hand; he says, "John Hunter, and more recently Cleland, have said that these muscles flex the first phalanges and extend the two distal ones, whereas Quain and Thomson say that the interossei, assisted by the lumbricales, flex the first phalanges, and at the same time, by their union with the common extensor tendons, extend the other phalanges. Duchenne states that the interossei extend the second and third phalanges, and partly flex the first." After reading this over many times with a view of finding out wherein lay the antagonism between the views of Hunter and Cleland on the one hand, and Quain and Thomson on the other (an antagonism undoubtedly suggested by the construction of the sentence), we were forced to the conclusion that it existed only in the imagination of the writer, and that the views of all these authorities coincide with those of Duchenne in this matter. Our own investigations have convinced us that the interossei are mainly flexors, and the lumbricales extensors, although the former have besides their osseous insertion a slight connection with the extensor tendons; together they produce the peculiar combination of flexion at the proximal joint with extension of the two distal joints, which is brought into play in the production of the upstroke in writing.

Among other matters which need correction, we may notice that the anterior interosseous artery passes behind and not in front of the pronator quadratus muscle as herein described; that the index is not a glossarial one, although it is described as being so both in the preface and table of contents; that the bibliography in many instances refers to old editions of standard works, instead of the latest ones, this being notably the case with Holden's *Manual of Dissection*, Ellis's *Demonstrations of Anatomy*, Heath's *Manual of Practical Anatomy*, Cleland's *Directory for the Dissection of the Human Body*, Gray's *Anatomy*, Wilson's *Anatomists' Vade Mecum*, and many others.

The prominent meritorious parts of the work are the description of the fasciæ of the perinæum, the practical surgery hints relating to the same region, the chapters on the homology of the bones, muscles, and ligaments, the summaries of dissections, the prominence given to those important but little noticed structures, the veins, and lymphatics, the woodcuts showing frozen sections of different parts of the body and the abnormalities of the blood-vessels most frequently met with, and the diagrams showing the arrangement of the synovial sheaths at the wrist and palm of the hand. The author everywhere encourages the student to thoroughness and completeness in his dissection, telling him not only to lay the bones bare, but to study them when so denuded, and not even then to think he has finished his work, but to make sections of them so as to observe the varying thickness of the cartilage which encrusts their ends, and the arrangement of the structure of their interior. We sincerely wish we could get students to follow the advice thus given, but fear we shall look in vain for this "consummation devoutly to be wished" as long as the passing an examination is regarded as the highest good to be derived from the study of anatomy.

The work has such distinct merits along with such obvious defects that it is difficult to give a summary judgment regarding it; but we think we are justified in saying that it is creditable both to the industry and knowledge of Mr. Reeves, as well as to the anatomical School of Great Britain.

First Aid to the Injured. Five Ambulance Lectures. By DR. FREDERICK ESMARCH. Translated from the German by H.R.H. PRINCESS CHRISTIAN. Smith, Elder & Co. 1882.

THE word "ambulance," as used at present in England and the United States, has attached to it a meaning which it does not possess abroad, for among the French, from whom we have adopted the expression, it is always applied to a field hospital attached to an army and moving with it, just as the well known *ambulances volantes* of Larrey were field hospitals fitted for very rapid movements. With us the term has a much wider significance, and we even apply it to the *conveyance* itself by which sick or wounded people are moved about. Of recent years it has still further come into vogue as a designation for those courses of lectures which have been initiated with the view of instructing the general public in a knowledge of what is to be done in cases of accident and sudden illness before the arrival of a medical man. The credit of starting these "ambulance lectures" belongs to the St. John Ambulance Association, a branch of the Order of St. John of Jerusalem in England, the modern representative of an old hospitaller order. The headquarters of this Association are in London, at St. John's Gate, Clerkenwell; but, so successful has the movement become that there are very few important towns and districts in the kingdom which do not possess local centres affiliated to the head office, and at which instruction in the preliminary treatment of those injured is given.

While on a visit to England, Professor Esmarch became acquainted with the work of the St. John Ambulance Association, its mode of teaching and its aims, and he was so struck with the simplicity of it and the necessity for it, that, on his return to Germany, he determined to use his influence to spread a similar movement there. With this view, he started at Kiel, on 5th March, 1882, a "Samaritan Society," preferring this title to that of "ambulance," which, as already explained, would have little or no meaning for continental nations. The objects of this "Samaritan Society" are the establishment throughout the country of "Samaritan schools," at which classes will be formed and instruction given in *the right kind of aid* to be given in accidents and other emergencies before the doctor's arrival. The school at Kiel has proved itself a great success, and to the members of it Professor Esmarch delivered last winter a course of lectures, which the British public are fortunate enough to have placed within their reach by the labours of H.R.H. Princess Christian, who has translated them from the German into English.

The lectures are five in number, and the first one is taken up by some preliminary remarks on the object of the instruction to be given, followed by a short and concise outline of the structure and functions of the human body. The second lecture deals with injuries and the subject of hæmorrhage, while admirable advice is given as to how a non-professional person can render aid to the wounded. At the same time, a short account is furnished as to how wounds heal and how a surgeon treats them, and this last point enables Professor

Esmarch to introduce an excellent résumé of Lister's antiseptic system and the principles on which it is founded. Fractures, dislocations, sprains, and burns form the subject matter of the third lecture, while under the fourth one there fall to be considered cases of poisoning and those demanding artificial respiration, such as drowning and suffocation. The concluding lecture treats fully of the transport of the sick and wounded, either by stretchers, or by rail, or by country carts, or even by improvised methods when no ambulance *materiel* is available.

It is impossible not to speak in terms of the highest praise of the manner in which Professor Esmarch has treated all the above subjects. Freeing his language from all technical phraseology and expressing himself in simple words, he has brought the instruction down to the level of the most ordinary intelligence. And yet the book will well repay perusal by professional men, as it is replete with useful hints and suggestions, and it is another illustration of the old saying, that none can so well popularise a scientific subject as those who are thoroughly at home with what they teach. H.R.H. Princess Christian (who is herself a member of the St. John's Ambulance Association, and has not only attended a course of ambulance lectures, but has also by examination obtained the certificate of the Association) is to be congratulated on her share in the present work, for she has produced an admirable translation, faithful in all respects, and yet very readable. There can be no doubt that in Germany this little book will help on the spread of ambulance work, and its English translation will be of great service in diffusing throughout our midst knowledge of the highest utility, the value of which is enhanced by its being the teaching of one who has in many other ways done so much for surgery, and whose name has been for so long specially associated with ambulance work in war.

Index-Catalogue of the Library of the Surgeon-General's Office, United States Army. Authors and Subjects. Vol. III. Cholecyanin—Dzondi. Washington: Government Printing Office. 1882.

The third volume of this gigantic undertaking has now been received. The second volume has its preface dated 1st June, 1881, and the third 27th June, 1882. The rate of issue is somewhat slower than was anticipated.

In former notices we have explained that the object of this

Index-Catalogue of the library, is to give the whole of the authors of books and pamphlets in alphabetical order together with a classified list of subjects, with the titles of books thereon repeated; but, in addition to this, the contents of the medical periodicals in the library are indexed according to their subjects, so that in this way every medical library in the world acquires an enormous additional value from each successive volume of this *Index-Catalogue.*

Some idea of the vastness and completeness of the enterprise may be gained from noticing the entries under "CHOLERA" in this volume. These extend from page 1 to page 152—large quarto pages, with double columns, containing much small type. Under the same heading we find the literature classified as regards various countries and towns, and amongst them we find "Glasgow."

It may interest our readers to have the entries given as they appear:

"GLASGOW.

"**Adams** (A. M.) Report upon cholera as it appeared in the 17th district of the City parish of Glasgow during the months of November, December, January, February, and March, 1848-9. Edinb. M. & S. J., 1849, lxxii, 285-314. *Also, transl.:* Hosp.-Medd., Kjobenh., 1850, iii, 496-518. *Also,* Reprint.—**Adams** (J. M.) Observations on the epidemic cholera of 1848-9, chiefly as it prevailed in the 13th medical district of the City of Glasgow Parish. Month. J. M. Sc., Lond. & Edinb., 1849, ix, 1012; 1087. *Also,* Reprint.—**Auchincloss** (W.) Report of the epidemic cholera as it appeared in the town's hospital of Glasgow, in February and March, 1832, with cases and observations. Glasgow M. J., 1832, v, 113-140.—**Bryce** (C.) An examination into the etiology of cholera, founded on the phenomena of the disease, as exhibited during its prevalence in Glasgow and suburbs. *Ibid.*, 262-298.—**Cholera** in Glasgow. [Edit.] Glasgow M. Exam., 1831-2, i, 286-288.—**Cleland** (J.) Conspectus of cholera in Glasgow; being a table of weekly cholera cases occurring in the whole district. Edinb. M. & S. J., 1833, xxxix, 503-506.—**Crawford** (J.) Observations on the present epidemic of cholera in Glasgow. Glasgow M. J., 1854-5, ii, 59-77—**Easton** (J. A.) Report of diseases among the poor of Glasgow, during the months of May, June, and July, 1832. *Ibid.*, 1832, v, 442-452.—**Lawrie** (J. A.) Reports of the Albion street cholera hospital. *Ibid.*, 309; 416. *Also,* Reprint.—**M'Etheran** (J.) Report of the epidemic of cholera in the 16th district of the city of Glasgow, from December 20 to February 21, 1848-9. Month. J. M. Sc., Lond. & Edinb., 1848-9, ix, 1165-1166.—**M'Gregor** (R.) Two clinical lectures delivered upon the cases of cholera treated in the Royal Infirmary of Glasgow during the late epidemic. Lond. M. Gaz., 1849, n. s., ix, 141-145.—**Moore** (S. J.) Recent cases of cholera at Ibrox. Glasgow M. J., 1867, n. s., i, 386-403.—**Statistics** of the malignant cholera in Glasgow, 1848-9. Lond. M. Gaz., 1849, n. s., viii, 611-613.—**Watt** (G.) On the origin and spread of malignant cholera in Glasgow and its neighbourhood. Glasgow M. J., 1832, v, 298; 384."

Applying a local test in this way shows how important for purposes of reference such a catalogue is. Curiously enough this same test brings out one of the weak points in relying on such a catalogue for the bibliography of the subject; for we find that the well known book on *Cholera* by Dr. Christie of this city does not appear in the list, evidently because it does not happen to be contained in the library, for it does not appear under Dr. Christie's name either. The authors of the catalogue warn those who use it that omissions of this description in the bibliography of a subject must inevitably occur.

We have looked over this volume very carefully, and we cannot help expressing our admiration at the great accuracy with which it has been produced, so far as we have tested it; and we heartily congratulate Dr. Billings on the progress of the work.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

GLASGOW ROYAL INFIRMARY.

REPORTS UNDER THE SUPERVISION OF DR. J. A. ADAMS.

FROM DR. STIRTON'S WARD.

CASE OF PELVIC PERITONITIS—[Reported by Andrew Semple, M.B., C.M.]—A. F., æt. 22, weaver, admitted 27th December, 1882.

On the 17th November last patient gave birth to a living female child; she made an excellent recovery, and in four weeks returned to her work, but after a week she was obliged to confine herself to the house on account of a pain which started in the left iliac region and gradually extended across the hypogastric to the right iliac region; accompanying the pain were considerable thirst, anorexia, diarrhœa (attributed by patient to a mixture got from a doctor), nausea and vomiting, suppression of milk, and the formation of an abscess in the left breast, which subsequently burst and then healed up.

On admission these symptoms were still present, with the exception that constipation had taken the place of diarrhœa. Pulse 108, small and wiry; respirations, 28; temperature, morning 100·2°, evening 103·2°; face flushed; tongue furred. The pain in the hypogastrium was relieved somewhat when the thighs were flexed on the abdomen, and aggravated by pressure, even of the bed clothes. On palpation, in the hypogastric and iliac regions, numerous irregular indurations could be felt all over.

On 29th December twenty-four half-grain powders of *hydrarg. c̄ creta* were ordered, one every two hours during the day, and every four hours at night. In five or six days afterwards the indurated masses were found to have softened considerably, on the 10th January the powders were repeated as before, and on the 15th the indurations had entirely disappeared, and the patient was perfectly convalescent.

I beg to call the attention of medical men to the method of treatment (detailed above) of an affection whose stubborn resistance to the action of almost any remedial measures has already become proverbial. The exudation into the broad ligaments, &c., was of the usual fibrinous character, and gave the impression to the hand of almost stony hardness. I have found that gray powder in such minute doses is, besides, a powerful stimulant to the action of the kidneys even during the nephritis following scarlatina, and, what is somewhat extraordinary, its use in such doses is not contra-indicated in the albuminuria which attends such cases. At any rate I should like to see reports on similar instances from the readers of this *Journal*.—James Stirton, M.D.

WESTERN INFIRMARY.

Reports under the Supervision of J. LINDSAY STEVEN, M.B.

From Professor George Buchanan's Wards.

CASE OF ABSCESS OF THE HEAD OF THE TIBIA.—R. M., æt. 25, engineer, admitted 13th December, 1882, suffering from an affection of the right leg, the chief manifestation of which is pain in the tibia, with some enlargement and nodulation of the bone.

The following is the clinical history of the case:—The disease commenced about twelve years ago, and, although he is not aware of ever having injured the limb in any way, he remembers that he first complained of pain in the leg, just below the knee, after a game at cricket. The pain gradually became more and more severe, until at length it was associated with the formation of a large abscess on the front of the leg. The abscess was incised in several places, and for a period of fifteen months remained open, discharging pus and at times small fragments of bone. At the end of this time he made a good recovery, and remained well for a period of four years, although it must be noted that, while employed as a grocer, and thus standing a great deal, he was occasionally roused up at night by pain in the right leg near the ankle. The second severe attack, however, came on after a bad cold, and its chief symptom was pain in the head of the right tibia, worst at night. This attack confined the patient to the house for six months; and, when at length he was again able to go about, the limb was found to be slightly flexed and could not be straightened. Two years after this he had a similar attack of pain lasting about six months, since which he has remained well until the month of August last, when all the old symptoms reappeared. After staying at home for a period of four months, he was admitted to hospital as noted above.

On admission the right leg was found to be slightly flexed at the knee-joint, and complete extension could not be effected. Numerous cicatrices were seen in front of the tibia, and in these situations the skin was firmly bound down to the bone. The anterior surface of the lower end of the tibia was rough and nodulated. There was some bulging at the sides of the ligamentum patellæ, and also at the sides of the rectus tendon. The circumference of the right knee was greater than that of the left, and the upper part of the leg was considerably increased in thickness. Firm pressure with the point of the finger upon the upper part of the tibia gave pain. The family history was satisfactory.

19th December.—To-day, the patient being under the influence of chloroform, Dr. Buchanan, by means of a crucial incision, exposed the tibia at the site of greatest intensity of the pain, just below the head. By means of a trephine about the size of a threepenny piece, a cork shaped piece of bone, about $\frac{3}{4}$ inch long, was removed, after which a few drops of pus flowed from the cavity. The internal end of the removed piece of bone was distinctly cup-shaped and lined with a very typical pyogenic membrane, thus verifying the diagnosis of abscess of the head of the tibia.

21st December.—Patient doing well, and pain greatly relieved since the operation.

22nd December.—Improvement still marked. The wound looks healthy, and the cavity in the bone is beginning to granulate up.

15th January.—Wound almost healed and pain gone.

PRIVATE PRACTICE.

By A. Napier, M.D.

PURPURA.—M. C., æt. 14, was first seen by me on the evening of Monday, 3rd October, 1881. A week previously she had slight sore throat. On Sunday she had walked some distance into the country and come home much exhausted. On going to school on Monday she felt tired, and suffered from pains in the limbs, which caused her to walk very slowly; about midday these pains became so severe that shortly afterwards she had to be sent home. When seen in the evening there was comparatively little constitutional disturbance; "the patient eats and sleeps well but is 'weakly,' and is apparently overworked at school. Tongue clean; pulse 85; temperature, 100·2° F. The legs are very decidedly swollen and tense from knee to ankle, and a little above the knees, on the outer aspect of each thigh. These parts are firm and resistent, and do not pit on pressure; the slightest touch or movement causes most intense pain. Scattered over the swollen parts, on both legs and thighs, are numerous purpuric spots, varying in size from a mere speck as large as a pin's head to patches as large as sixpenny pieces. These spots are vivid red in colour, and do not disappear on firm pressure. There is no heat or itching. The spots appeared in the course of Monday afternoon and evening. There are no pains in the joints; the gums and mucous membranes generally are healthy, and there are no hæmorrhages." The patient was ordered a mixture of tincture of perchloride of iron and chlorate of potash, and was directed to remain strictly in bed.

"*4th October.*—Spots darker in colour; swelling and general condition unchanged, with the exception that the pain on handling the parts is considerably less. Urine normal. No fresh spots. Pulse 84; temperature 98° F."

"*6th October.*—Better generally; spots on legs yellowish-green, like a fading bruise. Pain on touching or moving legs nearly gone. In both parietal regions, however, at symmetrical spots, are slightly painful circular swellings, about an inch in diameter. At symmetrical spots on both elbows also, just behind the outer condyles, are swollen bruised looking spots, yellowish-green in colour, and painful to pressure. The back of the right hand is swollen, painful to pressure, but not discoloured. All these swellings were discovered yesterday evening and last night."

"*7th October.*—To-day the back of the left hand is swollen and tender, like the right; other swellings much smaller and less painful. Spots on legs fading fast, and swelling has nearly disappeared."

In a few days more all these morbid appearances had gone, but it was some time before the patient regained her former strength.

Remarks.—This was not simple purpura; nor was it purpura urticans, as was shown by the absence of heat, itching, or wheals; nor was it purpura rheumatica, as the swelling and pain were not in the joints, and the girl had no rheumatic history or tendency; while the healthy state of the mucous membranes and the absence of hæmorrhages, forbade the idea of scurvy. It seems to have been a neurotic affection, due to exhaustion from over-fatigue, the chief evidence of this being the strikingly symmetrical character of the phenomena, the purpuric spots and the acute and circumscribed œdema probably ranking among the other symptoms of debility and exhaustion. On this view, the treatment by rest and tonics, especially iron, commends itself.

MEETINGS OF SOCIETIES.

GLASGOW MEDICO-CHIRURGICAL SOCIETY.

SESSION 1882–83.

MEETING III.—1ST DECEMBER, 1882.

DR. GAIRDNER, *President, in the Chair.*

THE PRESIDENT read "Résumé of Koch's Observations on the Tubercle Bacillus," by Dr. G. A. Heron, London. (See page 94.) The bacillus was also exhibited by Dr. J. Lindsay Steven and Dr. Newman.

Dr. Newman said that, at the close of the summer session, Dr. Coats and himself had arranged to carry out a series of investigations, not only on the tubercle bacillus, but on the organisms of other infectious diseases. They had already got the necessary permission from the Home Secretary to conduct the experiments, and only awaited the termination of Dr. Coats' other engagements to make a beginning. There were three methods of staining the preparations of the tubercle bacillus—viz., Vigort's, Gibbes', and Ehrlich's methods. Koch's own method did not yield satisfactory results in his (Dr. Newman's) hands, though this might be partly due to his inexperience in using it. He preferred Gibbes' method, which was some time ago described in the *British Medical Journal* (Dr. Newman briefly described it). The experiments which he had made in this enquiry did not entitle him to say much on the subject. He had examined the sputa of 50 or 60 different patients, and had *post-mortem* examinations in a number of these. It was somewhat peculiar to these cases that in the sputa of the same patient the bacillus was sometimes present, and in abundance; while on other days it was entirely absent. It was thus not advisable to depend on one or two examinations; one must have perhaps six or seven. In one case of chronic bronchitis he detected the bacillus. The man died of heart disease; and in the lungs he could detect no tubercle, though it would be rash to say that none existed.

Dr. Steven said that in Leipzig, from which he had lately returned, stress was laid on the necessity of having the *post-mortem* examination made within a few hours of death; otherwise the bacillus would not be found. He was not aware whether the principle here involved applied to the sputa; but the presumption was that to obtain reliable results it must be fresh. Dr. Steven said he preferred Ehrlich's method of staining, which he proceeded to describe.

Dr. Macewen queried whether any of the investigators had found tubercle in the joints? The most important practical lesson from the subject seemed to be, that if the organism retained its vitality in dried sputa, it would be necessary to carry out rigorous sanitary precautions in medical wards of hospitals, as well as in private houses.

Dr. Coats said that it appeared to him that these researches of Koch had put the copestone on the general views which had gradually been developing as regards tubercular disease. When he (Dr. Coats) began his pathological experience one thing had strongly struck him, viz., the apparent incompatibility between such cases as he would class under "acute tuberculosis," and cases of phthisis pulmonalis. He could not see how the two diseases were linked together by any pathological nexus. This made him exceedingly chary in applying the term "tubercular" to conditions generally so named. Acute miliary tubercle, he could see, seemed to be due to the presence of a poison in the blood. Phthisis, on the other hand, was universally believed to be due to some constitutional taint. In the *post-mortem* room Dr. Gairdner and himself had not a few discussions on the question constantly arising—Is this tubercle? In tubercle he could see nothing but the presence of some poison, and he came to regard it as a definitive infective disease. In regard to phthisis pulmonalis he was in a maze altogether, not being able to regard it as merely an inflammatory condition, but not seeing any pathological connection it had with acute tuberculosis. All this uncertainty had been changed by the researches of Klebs and Cohnheim and Koch. These men had proved beyond doubt that phthisis pulmonalis, as well as acute miliary tubercle, were diseases due to the action of a morbid poison, and to be placed in the same category as syphilis and leprosy, and the group of zymotics. This simplification of the problem had conferred a great mental boon on him. It reconciled the views of Laennec with those more recent views. He could now meet and shake hands with Dr. Gairdner; and it was only fair to admit that the change of position was his own, and that his conclusions, based exclusively on what appeared to be pathological data, stood corrected by the more mature and more extended views obtained from Dr. Gairdner's clinical experience. There were still some questions which remained to be investigated. One of these was in regard to the alleged heredity of phthisis. It must be remembered that in nearly all, if not all, the class of diseases with which phthisis could now be associated, there was a predisposition to them in certain families. This was the case in typhoid fever, diphtheria, and others. Some persons exposed to the contagion of typhus were sure to succumb to it, whilst some were almost proof against its attack. Take a hundred animals and inoculate them equally with the poison of splenic fever, or chicken cholera, some of the hundred would be found to have resisted the poison. Algerian sheep resisted inoculation with anthrax virus in small doses, though other species did not possess the same immunity. Cochin-China fowls resisted chicken cholera when inoculated with it. There were instances of immunity from the attacks of certain organisms undoubtedly hereditary. In regard to his own observations, he had examined the sputa in a large number of cases, and found the tubercle bacillus in a considerable number. He was not yet able to say in what kind of cases the organism was to be met with. He had not found it in any of the tissues, probably from chemical changes in the tissues interfering with the staining process. In regard to the vitality of the organism in dried specimens, the process of drying prevented further chemical change. Dust was therefore peculiarly dangerous.

Dr. M'Vail said, as bearing on the clinical aspects of the question, he had the pleasure of hearing read at the late meeting of the British Association in Worcester, a paper by Dr. Theodore Williams, of the Brompton Hospital for Consumption, giving the result of inquiries which he had instituted into the health of the nurses of the hospital for a long period of years. He could not charge his memory with the exact numerical results, but Dr. Williams gave evidence to show that there had been no infection from the patients with whom they were in such close contact, and no contagion from the atmosphere which they were daily breathing. Of the cases of consumption occurring in the nurses, which were relatively few, he gave a description, with a view to bear out that in all probability they were not infected in connection with the hospital.

Dr. Hugh Thomson pointed out that in speaking of phthisis as an infectious disease they must use the term in a sense somewhat different from its ordinary acceptation. The infective principle of the zymotic diseases gave in a very short time evidence of its existence and growth; while in phthisis the process of development was very slow. There were other points of difference. Phthisis contagion was less frequent; it appeared also from the paper read that when the contagion poison was inoculated in small quantity, the disease was chronic, and when in large quantity, acute. On theoretical grounds this would not have been expected, as the rapid growth of such organisms might have been expected to make up very soon for the smallness of the quantity inserted. But in any case they must guard against using the word "infective" as applied to this organism as meaning the same thing as in ordinary infection.

The President said that it appeared to him that the attitude in which their minds should, for the present, remain in regard to this subject, was one of expectancy and suspense of judgment. It would be extremely rash to dogmatise on the facts before them as to what would come out of this great discovery, which, he had a firm conviction, was one of great interest and far-reaching consequence. Koch's own attitude on the facts he had laid bare was in every way admirable. On these facts he had not speculated; he had simply told them what the facts were, and was content to wait for further light to see how these facts would fit in with others which were already apparently established. For this reason he had made no strained effort to explain how the question of heredity was consistent with his facts, but was content to wait for the explanation. Of Koch's career, so far as known to him, he had the greatest admiration. Beginning as a country practitioner, he had managed, amid the harassments incident to such a life, to snatch time to conduct many experiments on these lower organisms. The report of these investigations found their way to the public, and eventually the German Government had offered him a place connected with the Sanitary Bureau of Berlin; and that place he believed Koch still filled. The removal of such a man from his original position, though it enriched science, had the effect of beggaring practice. Dr. Coats had alluded in very kind terms to the way in which his (Dr. Gairdner's) clinical experience had sometimes corrected the conclusions of the pathologist. Dr. Coats' difficulties had been felt by many thoughtful minds; and these difficulties reached their culmination when it was found that what was called tubercle by one, was denied to be tubercle at all by others. He had asked many years ago what were they to believe when an observer of repute said that the so-called "granulations" (otherwise and more commonly called miliary tubercles) in cases of tubercular meningitis were not tubercles at all; whilst Virchow affirmed that these were what he would select as the very type of tubercle? The discussion of the pathological aspect of the question at the Glasgow Pathological Society a year ago left the question at the point at which Koch's discovery found it. In regard to the hereditary question it would be rash to say much at present. Nothing could be more remarkable than the established facts as to the family predisposition to certain diseases, such as diphtheria and typhoid fever. The royal family was a conspicuous example of proclivity to typhoid of the most dangerous form. He knew of a family of six or seven children in which one member of the family after another, at a distance each time of some years, succumbed to diphtheria, till at last any throat ailment attacking any of the family was looked on by the parents as another member gone. In regard to tubercle, this fact of predisposition in families held good, and it was as part of the general question that this so-called heredity of phthisis was to be studied: and it was from this direction they should look for light to bring into harmony the great discovery of Koch with the facts already verified. There need be no hurry to solve the problem. They might rest assured that whatever was true in science would be permanent. The practical applications of Koch's discovery were to be looked for chiefly in two directions. First in that of *prevention.* This discovery imposed on medical men the necessity of looking to the observance of the most scrupulous cleanliness. They must have clean hospitals, clean wards and walls, clean rooms and floors. More than ever must this now be the order of the day. They must keep in view that no man was safe unless he got everything round him as clean and as pure as could possibly be managed. Another possible direction in which what they had learned might be applied (though the question was so obscure that probably even Koch would not push it), was in making experiments to ascertain whether the tubercle bacillus could be cultivated into a milder form. Were this possible the question might arise whether, as in the case of small-pox and anthrax, the milder form might be utilised as a prophylactic against the more virulent form. In the matter of cure, too, attempts must be made to apply the discovery. For the next year or two there would be a run on the indiscriminate use of antiseptics in the treatment of phthisis; and probably this would result in disappointment. But this ought not to discourage them, as they might feel sure that whatever residuum of utility there existed in the antiseptic treatment of phthisis would eventually be made clear.

On the motion of the President a cordial vote of thanks was given to Dr. Heron for his paper; and the President was authorised to acquaint him with the fact.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Funnel-Drainage in Anasarca.—This is a methed of removing large quantities of fluid devised by Dr. Straub of Tübingen. It is simpler than Southey's, and affords more relief in equal time. The apparatus consists of an ordinary glass filtering funnel, of about 2 in. diameter, with attached to the end of it an india-rubber tube, ⅛ in. in diameter, and long enough to reach to the floor. If the apparatus is filled with water, and the mouth of the funnel firmly applied to the skin of a patient lying in bed, while the end of the tube is immersed in a vessel on the floor containing a little water, it will be found to adhere firmly and act as a sucker; and when the funnel is applied over several small incisions or punctures in a case of œdema, the tube acting as a syhpon will keep up a continuous drainage of serum into the vessel. The force of the suction can, of course, be regulated by altering the level of the vessel, and the flow of fluid can be watched by a piece of glass tubing let into the india-rubber tube. The apparatus, if protected by a small cage or cradle, can be left on for any length of time, and is not displaced by movements of the patient if ordinary care is taken. Enormous quantities of serum have been drained off in this way. In one case of chronic Bright's disease there was drawn off in 2¼ hours, over 78 oz. (2270 ccm.); in 7 hours, over 96 oz.; and in 24 hours, 278 oz. (8050 ccm.); and in another case of extreme general dropsy from Bright's disease, nearly 43 pints (24,800 ccm.) was removed in 79 hours.—*Centralbl. f. Klin. Med.*, 1882, No. 25.—D. M'P.

The Treatment of Exophthalmic Goitre by the Subcutaneous Injection of Duboisine.—M. Desnes reports three cases of goître treated by the subcutaneous injection of the neutral sulphate of duboisine (½ to 1 milligramme daily according to the tolerance of the individual). In three cases the amelioration has been most marked: the projection of the eyeballs decreased; the palpitations became less severe; the general health improved, and the throbbing at the thyroid vessels became less.

On the cessation of treatment the amelioration diminished.

The experiments have not been sufficiently numerous as yet to determine whether the cure is permanent.

In two cases the remedy had to be stopped on account of the symptoms it produced, the patients complaining of a feeling of intoxication, cramps, and formication in the gluteal region and at the back of the thighs, &c.—*Lyon Médical. 24th December, 1882.*—J. A. A.

Endocarditis in Diabetes.—Among the inflammatory affections which may occur in diabetes, M. Lecorché enumerates endocarditis. He has met with it more frequently among women than among men, always in a chronic form, and in subacute or chronic forms of diabetes. It occurs only at an advanced stage of that disease, two or more years after its commencement. It is rather the duration than the degree of the glycosuria that influences it. The mitral orifice is the part affected, but in one case it was the aortic. Its presence is revealed by a murmur with the first sound at the apex, and by the irregularity and the intermission of the pulse. It is sometimes accompanied by atheromatous degeneration of the arteries. The endocarditis precipitates the course of the diabetes, and often causes death, either by general œdema or ascites, or by complication with acute hepatitis. It appears to be due to the irritation of the lining membrane of the heart by the prolonged contact of blood containing an excess of sugar.—*La France Médicale.* 14th March, 1882.—G. S. M.

Concussion of the Chest. *(Commotio thoracica. Brusterschütterung.)*—At the German Surgical Congress, held in June last, Riedinger (Würzburg), described experiments he had performed to demonstrate the effects of concussion of the chest as distinguished from contusion. "While in contusion there occur decided injury to the lung, rupture of vessels, &c., in concussion there is simply the "shock effect" (*erschütterungseffekt*) of the blow, without material complication. The injured person after receiving the blow makes a deep inspiration, followed by gasping expiration, which is again often succeeded by a sigh, turns pale, and shivers, and cold sweat appears on the forehead. Patients usually recover themselves quickly, but cases have occurred where death rapidly followed." Riedinger's experiments were performed upon rabbits so secured that the results would not be obscured by the counter-thrust of a solid supporting body, and means were employed to register alterations in respiration and in blood pressure. It was found "that a blow upon the chest caused the blood pressure to fall suddenly, and then return to the normal in leaps. After several blows the depression was more lasting. Respiration showed only slight alteration. . . . . *Post-mortem* examination in many cases gave entirely negative results, in some there were small, often punctiform extravasations in the lung, nearly all sub-pleural."

This sudden fall is supposed to be caused by irritation of the vagus nerve, and the more permanent depression to paralysis of the sympathetic branches lying within the area of disturbance. That the effects of a blow on the chest are widely felt is proved by the occasional occurrence in these animals of contusion of the liver.

"Commotio thoracica consists in a disturbance of the circulation in the central nervous regions, due in the first place to arrest of the heart's action from intra-thoracic irritation of the vagus; in the second, to more permanent diminution of *tonus* in the peripheral vessels."—*Beilage z. Centralbl. f. Chir.*, 1882, No. 29.—D. M'P.

Warm Water in the Treatment of Epistaxis.—M. Eugène Anguier gives an account in the *Lyon Médical* for 10th December, of a violent case of epistaxis which resisted all the usual remedies, such as cold, occlusion of nares, sinapisms, and the snuffing up of various astringents. Having exhausted his resources, he tried the effect of very warm water injected up the nostrils, and found that it at once arrested the hæmorrhage. The bleeding, however, recurred at intervals, but the patient's parents immediately had recourse to the warm water with the best results. He believed that the hot water, by reflex action, caused contraction of the bleeding vessels.—J. A. A.

Milk Indigestion in Young Infants.—For young infants who are unable to digest the mother's milk, as evidenced by the occurrence of hard curd in the stools and in the vomited matters, by colic, and general wasting, Dr. Curtis Smith strongly recommends the plan of treatment proposed by Eustace Smith. To clear out the undigested curd that may be in the alimentary passages, he first gives a full dose of castor oil. Then he prescribes two ounces of barley water, with half a drachm of liquor pancreaticus, to be given every three hours, just before the child is put to the breast. He records several instances of markedly good results. The rationale of the treatment is, that the starch granules of the barley water, when mixed with the milk, prevent the milk from forming hard curds. To get a proper mixture, the barley water must be given just before the milk.—*Chicago Western Medical Reporter*. April, 1882.—G. S. M.

Experiments Showing how Different Causes, but Chiefly Lesions of the Brain, and in Particular of the Cerebellum, may Determine after Death a General or Local Contracture.—Brown-Séquard has presented to the *Académie des Sciences* a note on some experiments of the above nature. His conclusions are:—

1. That a true contracture may take place a certain time after, as well as some time before death, and that this contracture may last for a long time and pass into the state of cadaveric rigidity, or disappear completely so as to permit the muscular irritability to be recognised.

2. That of the various parts of the brain, that which has the most power to produce the contracture after death, is the cerebellum.

3. That the preservation of the attitude in which they were before death, which has been observed in soldiers killed on the field of battle, depends not on the sudden occurrence of cadaveric rigidity, but on the production of a true contracture. —*La France Médicale.* 5th January, 1882.—G. S. M.

Subsequent History of Nerve-Stretching.—Dr. L. C. Gray forwards the following very candid note for publication:

"In my recent article on Stretching of the Facial Nerve, in the September number of *The Proceedings*, my second case, the young man of 22 years, was reported as cured. Since the publication of my paper, this patient, I regret to say, has relapsed. The relief was only obtained for two months and a half. Whether his present condition is as bad as it was when he first came to me, I have no means of knowing, for I have not seen him since the first days of the relapse.

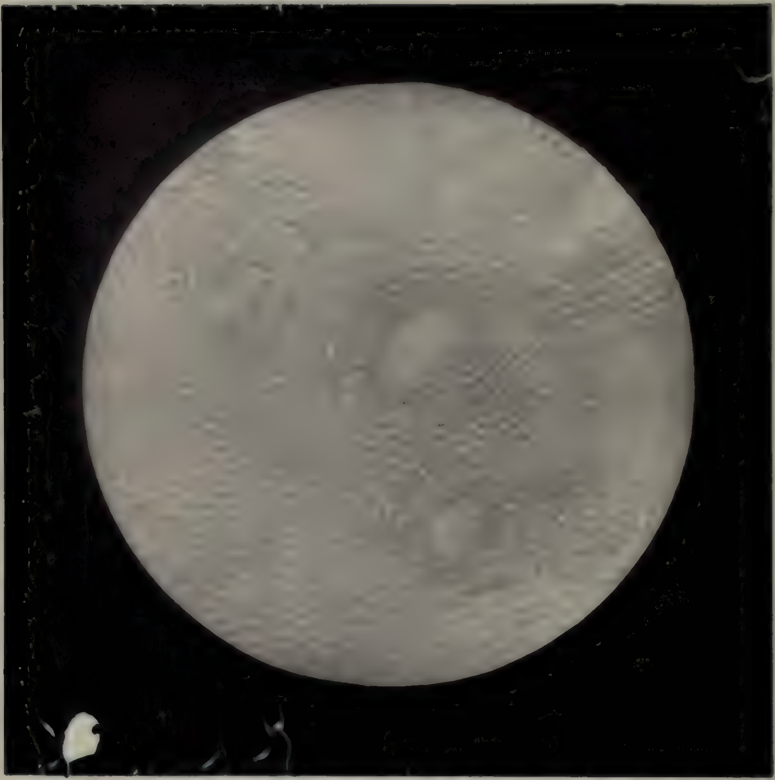
"I hope this addendum will travel co-extensively with my article and correct it. I have always suspected, and have frequently stated my suspicions, that a very material difference would be made in the percentage of nerve-stretching cures, were the gentle hand of correcting time not inhibited by hasty publication."—*Proc. of Med. Soc. of County of Kings*, 1882. [Dr. Gray's example is to be commended. If the after history of such cases were always, or even frequently obtained, the list of relapses after this operation would be largely increased.]

CREDIT TO WHOM CREDIT IS DUE.—Philip Doddridge (1702-1751), spoke of nerve-stretching, and recommended it as a religious stimulant in his *Zeal and Vigor in the Christian Race.*

> "Awake my soul: STRETCH EVERY NERVE,
> And press with vigour on."

—*Ibidem.*

PRESENTED WITH THE GLASGOW MEDICAL JOURNAL, MARCH, 1883



ADENO. SARCOMA OF MAMMA AMPUTATED BY DR. WHITSON.

PHOTO-MICROGRAPH OF SECTION.

Printed by the Woodburytype Company, from a negative taken by MR. ADOLF SCHULZE, *with Carl Zeiss' D.D., or $\frac{1}{8}$-inch focus objective.*

THE

GLASGOW MEDICAL JOURNAL.

No. III. March, 1883.

ORIGINAL ARTICLES.

A DEFENCE OF THE CONTAGIOUS DISEASES ACTS AND OF GOVERNMENT LOCK HOSPITALS.

By FREDERICK W. LOWNDES, M.R.C.S.Eng.

In the *Glasgow Medical Journal* for February, Dr. Alexander Patterson, replying to me, describes me as "a distinguished member of the Society for the Propagation of the Contagious Diseases Acts over the whole country." While thanking him for the complimentary expression, which I must disclaim, my position being the humble one of local honorary secretary, I regret to be obliged to correct an error in his first sentence. Ours is the "Association for Promoting the Extension of the Contagious Diseases Acts." We do not seek to propagate these Acts over the whole country, as will be seen from the following extract in the sixth report of the Association, published in 1875: "Under present circumstances, we do not aim at so wide and immediate an extension of the Acts as before, but the case of certain seaport towns not subject to the Acts, which are known to be hotbeds of disease introduced by sailors of the merchant service of our own and of foreign countries, is so glaring, and is attended with such disastrous consequences, that we feel it our duty to call for the special interference of Parliament to repress the evil."

Our wish is to carry public opinion with us, and though no extension of the Acts has taken place since 1869, we have been successful in defeating their repeal on six different occasions. In 1870 the Bill for repeal was "talked out," there being a large majority in favour of the Acts. The results of the three successive attempts to repeal them were as follows:—

Year.		Members Present.	For Repeal.	Against.	Majority for the Acts.
1873,	.	379	128	251	123
1875,	.	434	136	308	172
1876,	.	326	102	224	122

In 1878 the motion for repeal was again "talked out," and last year it was met and defeated by the "previous question."

While equally averse with Dr. Patterson to controversy with a professional brother, especially on such a subject, I feel it to be my duty to defend these Acts from the unjust attacks made upon them and upon those who support them. Of this latter Dr. Patterson gives a very striking illustration in his next sentence. He states that "a leading London Medical Journal which, for many a long year, had this very precept (*audi alteram partem*) as its motto, now declines to admit a single article in opposition to the Acts, and so declines because it takes a different view of the question. The reciprocity is all on one side." Now, the journal in question is evidently the *Lancet*, which adopts *audi alteram partem* as the motto for its correspondence column. If Dr. Patterson will look in the *Lancet* for 2nd December, 1882, page 962, he will find a letter from Dr. Ewing Whittle of this city, and in the same journal for the 18th November last he will find another letter from a Dr. Truman. Each gentleman is, as may be judged from their letters, as strong an opponent as Dr. Patterson himself, and each letter appears under this very identical motto, "*audi alteram partem.*" Again, if he will refer to the same journal for 28th October last, he will find among the "original articles," one from Dr. Rawdon Macnamara, of Dublin, who, though not quite so strong an opponent as Dr. Patterson, is by no means a supporter of the Acts, showing this in the article alluded to, and in his evidence before the Committee. Dr. Patterson's attack on the *Lancet* is therefore palpably unjust. It is true that the editorial articles are all in favour of the Acts, but Dr. Patterson will hardly dispute the right of an editor to take such view of a question as commends itself to his own judgment. Is the *Lancet* singular in this respect? On the contrary, the *Medical Times and Gazette*, *British Medical Journal*, *Medical Press and Circular*, all take the same view of the question. Are all these editors wrong and is Dr. Patterson alone right?

With regard to Glasgow, I have not disputed the reduction of brothels and prostitutes, nor the great improvement which has taken place as regards street solicitation. What I contended was, that Dr. Patterson was not justified in assuming a reduction of disease merely because the number of admissions in the Glasgow Lock Hospital had been diminishing of late years. I also asserted that it was not fair to assume that there were not sixty diseased women in the whole of the city and suburbs at any given time, because the sixty beds in the hospital were not filled. Dr. Patterson attempts to prove his case by comparing Glasgow with the places under the Acts, and by a quotation from Mr. James R. Lane's pamphlet showing the diminution of disease in those places. This is manifestly unfair, since it is comparing two totally different arrangements. I showed in my last paper in the *Glasgow Medical Journal*, for January, that before the passing of the Acts, Portsmouth, Devonport, and Winchester resembled Glasgow in having voluntary hospitals and wards for females, which wards were not filled. But that after the Acts of 1866 and 1869, the number of 162 beds in the Royal Albert Hospital Lock wards at Devonport, and of 120 beds in the Lock wards of the Portsmouth, Portsea, and Gosport Hospital were all filled. Gradually the effects of the Acts in the compulsory periodical examination of all the prostitutes in the districts, with the detention in hospital until cured of all such as were found to be diseased, began to manifest itself in the reduced number of occupied beds. Hence, Mr. Lane's statement, which Dr. Patterson has quoted very imperfectly and incorrectly, and which I will here give in full. "Another mode of showing very conclusively the diminution of disease is afforded by the fact that a large number of the Lock Hospital beds have been for some time unoccupied. At Devonport, where there are 162 beds, the daily average number occupied from 30th September 1868 to 30th September 1869, was 126·08, but from that date to the 30th September 1870, the daily average was only 76·03, and on the 22nd November 1870, only 46 beds were occupied. At the London Lock Hospital, with 150 beds, there has for some months past not been an average of more than 50 beds occupied. At Aldershot, with 100 beds, 40 beds were vacant in the middle of the present month. At Portsmouth, with 120 beds, only 84 are occupied. At Chatham, with 88 beds, only 63 are occupied. Now, as the whole number of beds provided was not more than sufficient to enable the provisions of the Act to be properly carried out at the end of 1868, the number of beds now vacant is excellent evidence of the good results which have been obtained." So that it is not a case of *post* *hoc ergo propter hoc* at all, the number of diseased women being enormously reduced, and as a necessary consequence the number of occupied beds being reduced too. Dr. Patterson, on the contrary, contends that because the 60 beds in the Glasgow Lock Hospital are only about half filled, therefore there are just that number of diseased women, and no more, in the whole of the city and suburbs. Let us see how far this is borne out by his own figures. He attributes the reduction of admissions into the Glasgow Lock Hospital, from 534 in 1870 to 394 in 1871, to "the effects of the Glasgow Police Act as put into operation in 1870." At page 414 (*Glasgow Medical Journal*, December), it is stated that there were 211 brothels in Glasgow in 1849, and in 1874 there were 204, which number was brought down to 38 in 1877; but how many there were in 1870 and 1871, the critical dates for Dr. Patterson's theory, we are not informed. As to the influence of the number of brothels on the frequency of venereal affections, so far as the records of the Glasgow Lock Hospital can show it, the following are the facts:—

Year.	Brothels.	Admissions to Lock Hospital.
1849, ...	211	200
1874, ...	204	436
1877, ...	38	422

from which it may be safely concluded, that the administration of the Police Act since 1870, though it may have reduced the number of brothels since 1874, has had no perceptible influence on the prevalence of venereal diseases; and further, that no connection can be established between the number of brothels and the admissions into the Lock Hospital. It is a case, in fact, of *non post hoc ergo non propter hoc*. Repressive measures, similar to those in force in Glasgow now, have been tried before in other places, the result being to increase disease among the number of prostitutes which will be found in any large city under whatever laws. I maintain that for every empty bed in the Glasgow Lock Hospital there are at least two diseased women in the city itself. If Dr. Patterson, Professor Dunlop, and Captain M'Call, will apply their minds and energies to discover all the diseased women in the city, persuade them to enter the Lock Hospital, and stay there until cured, they will be doing a much more useful and practical work than the Utopian task of trying to exterminate brothels and prostitutes.

Dr. Patterson may rest assured that, strongly though I support the Contagious Diseases Acts, yet equally strongly do I maintain that it is the duty of all attached to voluntary

Lock hospitals to make them as serviceable as possible. I have from the first laboured incessantly to fill our twenty-five beds, and I am glad to say that we have been fairly successful. The exclusion of students (whom it is hardly fair to call spectators) has been one factor of success, and a very important one too. The abolition of floor scrubbing was another. But a third, which I have not mentioned, was that, with the assistance and permission of our late head constable, Major Greig, and of his successor, Captain Nutt Bower, I have been enabled to furnish notes for the hospital to those police officers whose beats comprise streets in which are brothels. This last has brought many a poor creature to our hospital who otherwise might never have heard of it. I have never suggested that prostitutes, either in places under the Acts or elsewhere, have ceased to be women. What I contended was, that Dr. Patterson was not justified in accusing the Acts of unfairly affecting one sex and not another. They are not directed against one sex, but against a special class of that sex who make a living by prostitution. Among the male sex no such class exists.

Dr. Patterson has quoted many witnesses to prove his assertion that the Acts have a hardening effect on the prostitutes brought under their influence. As before, he has quoted the answers very incorrectly and imperfectly, picking out such parts as he thought proved his case, and ignoring the context. It is quite unnecessary for me to do more than indicate the three fallacies which his assertion involves. Firstly, it assumes that it is the Acts and not the life of prostitution which hardens the women. And yet we have most of us been brought up to understand that there is nothing so hardening, either to men or women, as vice, and this vice especially. Secondly, it assumes that hardened prostitutes are not to be found except in those districts where the Acts are in force. If the Rev. Flavel Cook was to visit the Lock wards of the workhouse in any large town or city not under the Acts, he would find many and many a specimen precisely similar to, if not worse than, any he sees in the Government ward of the London Lock Hospital. The Roman Catholic chaplain of our borough gaol describes the prostitutes who come again and again under his notice as "incorrigible women." Four years ago I gave evidence in a case where one of these incorrigibles stabbed a policeman in a fit of drunken frenzy. She had been forty-seven times in gaol! Thirdly, so far from the Acts being the cause of hardening prostitutes, their effect is in every way to prevent them from becoming hardened. The special police and visiting surgeons take every pains to prevent women and girls from coming on the register at all, if they can induce them to change their lives at once. Failing this, whenever an opportunity occurs, each woman is reminded, that if she will give up her vicious life she will be removed from the register without any difficulty. If found diseased and sent to hospital, every effort is made by the chaplain and matron to induce her to return to her friends or enter a "home." Should she elect to do either of these latter her expenses are paid by the Government; but in any case, on leaving the hospital cured, she is at perfect liberty to go wherever she pleases, and unless she resumes a life of prostitution no police officer will dare to interfere with her. All these are facts which cannot be controverted; and I fearlessly maintain that, if a prostitute becomes hardened it is not in consequence, but in spite of the Acts.

I freely admit having overlooked Dr. Patterson's replies as to the state of the women on admission to hospital, and as to the number of males admitted in Glasgow. As I had only the opportunity of a rather hasty perusal of his evidence this is not surprising. His argument is a very extraordinary one. In the rural districts they (*i.e.*, the prostitutes) will remain out, he supposes, "so long as they can keep their feet to earn their bread, but those that are in the town, the moment they know they are infected with disease, I believe, come to us." So that, according to Dr. Patterson, a woman knowing that she is diseased goes on spreading disease so long as she can keep her feet to earn her bread. Is any woman really driven to prostitution to earn her bread? Surely any woman, especially any Scotchwoman, who still retained her native sense of modesty so much as to shun examinations and spectators, would rather enter the workhouse, or even starve, than prostitute her body for gain. As for the number of males admitted, Dr. Patterson's reply is very meagre: "In the Royal Infirmary there is a male Lock ward, which I myself had charge of for a number of years. It takes in about 125 to 150 male patients in a year." A sad contrast, truly, to the elaborate statistics of females. I was enabled to give the Committee returns of both males and females from 1834 to 1881, which showed a most remarkable preponderance of males over females every year except one, though the number of beds is precisely the same. Our male wards were always full, while the female wards were half empty. Again, I was able to give them returns of our Seamen's Dispensary, opened in 1877, which showed a yearly average of 900 new patients. Hence some idea was given as to the prevalence of disease among men in Liverpool. As for Glasgow, the Committee were compelled to report that "no satisfactory evidence was given as to the amount of disease amongst the male population, an element without which it is absolutely impossible to measure the real amount and intensity of venereal disease in any locality, or the success of any system for its diminution."

As to giving chloroform, I have no wish to dictate to Dr. Patterson what his practice should be, I would merely reproduce what he stated at first. "In a very few cases of young girls, with acute inflammation of the parts, chloroform is administered prior to using the speculum. It can be well imagined that, if one persisted in introducing an instrument in such circumstances, without an anæsthetic, the girls would not be likely to remain long in the hospital." Quite so, but who ever heard of such a practice? It was never done in our hospital, nor by any surgeon under the Acts, nor, I should think, by any surgeon in his senses. Whether it is proper practice, even with an anæsthetic, for the economical reasons given by Dr. Patterson, I must leave others to decide.

I repeat that the periodical medical examination of prostitutes was intended to benefit, and does benefit the latter The Acts were passed for "the better prevention of Contagious Diseases at certain naval and military stations." This is the title of the Act of 1866, and is the fair way of stating the case. The first persons to reap the benefits of the Acts were the unfortunate women who, instead of being left as those whom Dr. Patterson has described as "remaining out so long as they can keep their feet to earn their bread," were at once examined and found to be diseased, and at once taken into hospital there to be kindly treated and cured. If this is not saving these women from untold suffering, I know not what is. Let us see what is the experience of Mr. James Lane, whom Dr. Patterson describes as "a most respectable pro-Acts man," but to whom I prefer to give his proper titles of Consulting Surgeon to St. Mary's and the London Lock Hospital. In the pamphlet to which Dr. Patterson has referred, he says, at page 13:—

"The good effect of legislative supervision in gradually lessening the *severity* of the disease, does not admit of numerical expression; the alteration in its quality, however, is quite as strongly marked as the diminution in quantity, and is of equal, if not greater, importance. The severe and shocking cases, which are so frequently seen in the wards of voluntary Lock Hospitals, are almost unknown in the hospitals receiving patients under these Acts. The more important form of disease—viz., syphilis, soon becomes singularly mild in character, and quickly amenable to treatment, both in its primary and secondary manifestations; while a considerable majority of the patients now admitted are suffering solely from the milder form of disease, or gonorrhœa. I can testify to this from personal observation at the London Lock Hospital, where women have been received under the Contagious Diseases Acts since the passing of the first Act in 1864. When patients are first admitted from a new district the cases are quite equal in severity to those which I am accustomed to see in the voluntary wards of the same Hospital, and nothing can be more striking than the alteration which takes place in this respect after a district has been for six months subjected to compulsory inspection. This is a fact of great interest, and tells strongly in favour of the system; it is one of which even those who have interested themselves in the results of these Acts are not yet, I think, sufficiently aware."

Dr. Patterson admits that these women would make their way to a camp established on Ailsa Craig or on the Bass Rock, "if boats were supplied by the Admiralty." I did not suggest the latter, as I am sure that the women would find their own ways and means of getting there. What were the reasons why the Acts were first passed? They are given in the concluding paragraph of the report of a Select Committee of the House of Commons appointed in 1862 to inquire into the prevalence of venereal disease in the army and navy. "Your Committee have refrained from entering into the painful details which have come to their knowledge of the state of our naval and military stations at home as regards prostitution. These facts are so appalling that they feel it a duty to press on the Government the necessity of at once grappling with the mass of vice, filth, and disease which surrounds the soldiers' barracks, and the seamen's homes, which not only crowds our hospitals with sick, weakens the roll of our effectives, and swells the list of our invalids; but which, surely, however slowly, saps the vigour of our soldiers and our seamen, sows the seeds of degradation and degeneracy, and causes an amount of suffering difficult to over-estimate." Dr. Patterson, forgetting that two wrongs don't make one right, cites the case of our sailors importing disease to Pacific islands. From whom did our men get the disease? From these women who surround the seamen's homes.

I can assure Dr. Patterson that I did not overlook the sobriety of the Hebrews, and that I am fully aware how greatly intemperance tends to aggravate prostitution and disease. But I cannot agree with him in thinking that sobriety, or even total abstinence, would extinguish the sexual passions to such an extent as to prevent syphilis more than all the Contagious Diseases Acts in the world." In private practice I have had many total abstainers suffering from syphilis and other venereal diseases. The Turks, too, are a sober nation, but I have yet to learn that the sexual passions are less in them than in other nations.

With regard to the title of "Queen's women" given to the prostitutes in protected districts, it is, I think, very undesirable that those who know better should repeat a term so offensive, disloyal, and misleading. If a woman chooses to adopt such a life no law can prevent her; but the women in protected districts have no more the sanction of the Queen or Government for their immoral lives, than have the prostitutes in Glasgow the sanction of the Lord Provost, or the thousands of prostitutes in London the sanction of the Lord Mayor. There is a form of recognition, the most dangerous of all, that of tacit recognition, or wilful ignoring of the evil known to exist. It is perfectly open to the parochial authorities, in every place where the Acts are in force, to prosecute any brothel-keepers; and the Acts have not removed one single law providing for the prosecution of such people. It is, therefore, manifestly unfair to charge the Acts with licensing a sin which exists to a very much greater extent in places where they are not in force. London, Edinburgh, Dublin, Liverpool, Birmingham, Bristol, and all our largest towns and cities, are localities where the Acts are conspicuous by their absence, and yet the "social evil" exists to an extent much greater than what obtains in protected districts. In Manchester and Leeds there are Police Acts for dealing with brothels and prostitutes very similar to that of Glasgow, but, so far as I can learn, with very unsatisfactory results.

Coming to the ages of the women in protected districts, and the so called hardening process, I have already shown that the latter is not in consequence, but in spite of the Acts. Dr. Patterson treats the Report of the Committee in a very cavalier way, and quotes from the works of the late Mr. Acton and Duchatelet (both strong supporters of the Acts) as if in refutation. The quotations are wide of the mark, as they relate to a totally different condition of things. The state of the women found formerly at Aldershot, Chatham, The Curragh, Cork and Queenstown, Portsmouth and Devonport, was such as did shorten their lives. It was not only the life of prostitution, but the life of exposure, dirt, and privation, which aggravated disease. Moreover, the age has been determined by the eradication of juvenile prostitution, and the fact that women who now lead a life of prostitution commence much later; seldom before 18. The late Mr. Holmes Coote was, it is true, an opponent of these Acts, but Dr. Patterson ought in fairness to have added, that Dr. G. E. Paget of Cambridge, Dr. Samuel Wilks of Guy's Hospital, and Mr. Timothy Holmes of St. George's Hospital, are all strong supporters of the Acts, and were also members of the Royal Commission.

I now come to a point as to which Dr. Patterson has recourse to Mr. Lawson's evidence to support inferences which, for the sake of clearness, as well as to illustrate Dr. Patterson's style of reasoning, I must give in full, though the quotations be rather long. At page 113 it is stated—

"There is more syphilis amongst women in the subjected than in the unsubjected stations. Take proof: Q. 1,800. 'BUT I UNDERSTAND YOU TO SAY THAT THERE WAS MORE SYPHILIS AMONGST THE WOMEN?' Inspector-General Lawson answers, 'YES.'

"Q. 1,804. '*A man runs more danger of catching true syphilis in the subjected stations than in the unsubjected stations?*' Inspector-general Lawson answers, 'TO THE EXTENT OF 36 AGAINST 33!' And in answer to Q. 1,803 the same high authority answers, 'YES, THAT IN 100 CASES YOU HAVE 36 CASES OF PRIMARY SYPHILIS AT THE SUBJECTED STATIONS, INSTEAD OF 33 AT THE UNSUBJECTED STATIONS.'

"Increase in proportion of secondaries to primaries in the stations under the Acts. Q. 1,817. 'INCREASE IN SUBJECTED DISTRICTS, 18·6.' Q. 1,818. 'IN THE UNSUBJECTED DISTRICTS YOUR INCREASE IS ONLY 11?' Inspector-general Lawson, 'THAT IS SO.'

"This, then, it appears, has been the effect of the Acts on the prostitutes. And it has increased the risk of the men's catching disease. So that the Acts have had the very opposite effect to what was delusively expected. Clearly the Acts are a great mistake, for the very evident reasons shown. The women have much less disease in the unsubjected stations, and in them Inspector-general Lawson admits that the men run less risk."

The capitals and italics in the above are Dr. Patterson's. It will be observed that he has emphasized questions 1,800 and 1,804, which were put by Dr. Cameron and Mr. W. Fowler respectively, and the answers; and after the latter he gives the answer to question 1,803, which was put by Mr. Osborn Morgan, but has omitted the question itself. In the evidence this question and answer stand thus:—Q. 1,803. "You mean that there is a slightly greater chance of catching the true syphilis, as opposed to pseudo-syphilis, in the protected stations than in the unprotected stations?" A. "Yes; that in 100 cases you have 36 cases of primary syphilis at subjected stations, instead of 33 at unsubjected stations."

It is obvious that Mr. Osborn Morgan's question, with the answer to it, coming between those of Dr. Cameron and Mr. Fowler, explained and limited the meaning of the replies to both, and it is not a little surprising that Dr. Patterson, while directing attention so pointedly to part of the evidence, should have suppressed the central part on which the true explanation of the whole depended. The real meaning of the evidence is that at the stations under the Acts, 36 per cent of the primary venereal sores which occurred were infecting sores, while at the stations never under them, 33 per cent only were infecting; as shown further on, the ratio per 1,000 of primary sores for the four years 1870-73, was, at the stations under the Acts 52·5, 36 per cent of which is 18·9; and at those, never under the Acts, the primary sores were 86·0 per 1,000, 33 per cent of which is 28·4, exactly 50 per cent in excess of that occurring under the Acts, so that Dr. Patterson's statement as to the increased risk of the men catching disease at the stations under the Acts is directly opposed to the fact.

The questions 1,817 and 1,818 have reference to the periods 1867 to 1872, and 1873 to 1878, the numbers for which are contained in the Return, No. 6 B. hereafter to be noticed. From 1867 to early in 1870, the Acts were gradually being applied to the different stations, and from that year onwards were enforced at all. Late in October, 1870, Lord Cardwell's order was issued, under which the pay of men while under treatment for primary venereal sores or gonorrhea was stopped, but it did not apply to those who had secondary syphilis; this led immediately to a large amount of concealment of the primary affection, but as it did not affect the admissions for the secondary, the proportion between primary sores and secondary syphilis, which had varied little up to 1870, was greatly deranged after that date, and could no longer be depended on as it previously had been. Another cause of derangement was the importation into one set of stations of secondary cases due to primary sores contracted in the other set. Every member of the profession is aware that a period varying from a month to a year or more may elapse between the appearance of the original sore and the constitutional affection which follows it. And during this interval a soldier may pass from a station under the Acts to one never under them, or *vice versa*, so that the secondary affection due to infection in one set is developed at, and accounted for in the other. Under these circumstances, it is obvious that a given number of men moving from a set of stations where syphilis is very prevalent, to another where it is much less so, will have among them a greater number in whom the secondary disease is impending, than a similar number moving from the latter to the former; and the effect of such transfers will be to lessen the number of secondary cases occurring at the stations where the primary disease is most prevalent, and to increase the secondary cases at those where there is less primary disease. This is what took place between the stations under the Acts and those never under them; and it was brought strongly before Mr. Stansfeld—who put questions 1,817 and 1,818—in reply to his further questions 1,819 to 1,821, but here again Dr. Patterson is reticent. For the reasons assigned, the increase in the proportion of secondaries to primaries at the period in question is of no practical value, as will appear from the actual course of the constitutional disease itself. The admissions for secondary syphilis at the stations under the Acts were, in 1867-72, 24·6 per 1,000; and in 1873-78, they fell to 22 per 1,000; in the stations never under the Acts, the admissions in 1867-72, were 29·2, and in 1873-78 they rose to 30·2—a result in direct opposition again to Dr. Patterson's conclusions.

Dr. Patterson's misquotations take so much space to set right, that I can only notice the more important parts of his paper. He denies the reduction of juvenile prostitution by the working of the Acts. But as will be seen from the following extract from the Committee's Report, it has been proved beyond dispute:—

"The returns of the department of the metropolitan police, which administers the Acts, have shown, since the introduction of the system, a continuous and great decrease in the number of juvenile prostitutes in the subjected districts. This evidence is borne out by the testimony of persons well acquainted with the condition of many of those places. As juvenile prostitution is the principal source by which the supply of fallen women is kept up, it is evident that the Acts, in diminishing the number of youthful prostitutes, are operating effectually to diminish the number of adult women abandoned to an evil life. Many cases were detailed to your

Committee in which young girls, who were either on the point of falling, or had already fallen, were rescued by the intervention of the metropolitan force, and restored to their families. But cases of this kind are few in comparison with the numerous class in which the deterrent influence of the Acts frightens young girls from vice. It is to be remarked, that while a constant decrease in juvenile prostitution has gone on in the subjected districts, the Committee of the House of Lords appointed to consider the subject and other kindred topics in 1881, states in its Report, dated 10th July, 1882, that 'juvenile prostitution, from an almost incredible early age, exists to an appalling extent in England generally, and especially in London.' Their Lordships attribute its prevalence mainly to certain specified causes. Every one of these causes has been proved to your Committee to be vigorously and effectively counteracted by the administration of the Contagious Diseases Act, so that the alleged reduction of juvenile prostitution in the subjected districts is borne out by the fact that the influences stated by the Committee of the House of Lords to be its principal source are deprived of much of their strength, where the administration of the Acts is brought to bear against them.

"The causes referred to are:—

"1. The want of parental control. This is remedied by the information which the police give parents as to the dangers of their daughters, and by the authority which the police exert for the reclamation of young girls.

"2. Residence in brothels. As already shown, it has been proved to your Committee that the police exert their power with excellent effect to prevent brothel-keepers from harbouring young girls.

"3. The example and encouragement given by girls slightly older. The deterrent influence of the system acts effectually against the temptation.

"4. The state of the streets 'in which little girls are allowed to run about and become accustomed to the sight of open profligacy.' Your Committee find that the Acts have much improved the condition of the streets, and repressed public disorder and indecency among fallen women, thus removing much of the bad example which was formerly to be seen in subjected districts."

I have already shown that the objects of the Acts were to prevent contagious diseases. The quotation from General Sir Henry Stork's evidence, given by Dr. Patterson, is the General's private opinion, and does not represent the object nor result of the Acts. Dr. Patterson must be aware that, if soldiers or civilians wish to be chaste from the highest motive of all, because it is their duty to be so, it will not matter to them whether they are stationed at Aldershot or Glasgow, at Dover or Ayr. To a really virtuous soldier it will not signify whether the prostitutes around the camp are diseased or healthy. It is therefore as absurd to suppose that the Acts tend to make men immoral, as it is to suppose that they tend to create prostitutes.

Dr. Patterson's remark about the "religious or moral instruction afforded by the chaplains, when the inspecting medical officer makes it a rule to supply them, *i.e.*, the women, with a syringe," etc., is in very bad taste. If a woman detained in the hospital wishes to give up her vicious life, she need never see the inspecting medical officer again. If the chaplain succeeds in making an impression upon her, every assistance will be afforded him by the medical officer and the matron. The "instructions" only apply to women persisting in a vicious life after every attempt to dissuade them has failed. In attempting an exposure of Government Lock Hospitals, Dr. Patterson has only exposed his entire ignorance of them, and the manner in which they are conducted.

I have disposed of the absurdity of the argument that the Acts apply to one sex and not to the other. Dr. Patterson's imaginary cases are not parallel. Let us take one, which is at least more so. If Dr. Russell (before whom I had the honour of reading a paper in defence of these Acts in 1876 at Sheffield) were informed that scarlet fever had broken out in a brothel, it would, I assume, be dealt with like any other case. If any difference was made, more care than usual would be taken to prevent the fever spreading, because it was a brothel. We should not be told that the prostitutes must be left till they apply for admission to a voluntary fever hospital. Nor should we be told that vicious men going there must take the full consequences of their own vice. And why? Because the public must be protected from scarlet fever. Perhaps some day it may be thought to be desirable to protect it from syphilis.

2, 3, and 4. It is not pretended for a moment that the periodical examinations made every fortnight provide absolute immunity from disease. What is contended is that it enables the visiting surgeon to detect disease early, and so prevent its spread on the one hand and its aggravation on the other.

MEDIATE CONTAGION.—Dr. Patterson says:—"*Mediate Contagion.—Now, I desire the reader's especial attention to this* *deliberate statement of Mr. Lowndes*, and leave him to form his own opinion as to Mr. Lowndes'—accuracy. 'Mr. James Lane, of the London Lock Hospital, Dr. Barr of Aldershot, and other experienced examiners, consider the *probability* of *mediate contagion as very* remote.'

"*Mr. Lane*—'I think it is impossible to deny the possibility of mediate contagion.' (2,395). 'It is quite possible, if a woman has connection with a great number of men (or any lesser number), and adopts no means of cleanliness, . . . but my knowledge does not enable me to say how infrequent it is, nor can any man's knowledge enable him to say how very infrequent it is.' (2,529 to 2,546).

"*Dr. Barr of Aldershot.*—The great spread of disease in this manner is frequently referred to by Dr. Barr. *A.* 1,482—'*A woman at the time is free from disease*, and the *fact of her communicating venereal disease to a soldier is not due to her unfitness. but to contagious matter which she retains within her, the result of intercourse with a diseased man.*' (1,481-1,795). 'Gonorrhœa, syphilis, and sores are communicated in that way, the woman herself remaining free.'

"*Question* 4,738 B.B.—'We have heard about "mediate contagion." You are aware that that is a very controverted point in the profession?' Dr. Barr answers, 'I have heard that it is so. I have my own opinion about it. I BELIEVE IN IT FROM ACTUAL PROOF.' In answer to question 4,742 Dr. Barr answers, 'Certainly. In this matter of mediate contagion, I am speaking of a woman having had intercourse with twenty different men, and not cleaning herself after connection. I say it is only fair to assume that, after having connection with a lot of these men, although she may be healthy, they being diseased, subsequently might disease a young recruit; *that is, by mediate contagion.* I SHOULD NOT HAVE ADVANCED THIS STATEMENT UNLESS I COULD PROVE IT.'" The italics and capitals are Dr. Patterson's.

I desire equally special attention to Mr. Lane's answer, as given in the *Blue Book* of evidence, and leave the reader to form his own opinion of Dr. Patterson's accuracy of quotation. The italics following are mine, to mark the words which Dr. Patterson has thought proper to leave out, or alter.

A. 2,395. "I think it is impossible to deny the possibility of mediate contagion; *but I cannot believe that it is frequent, and I believe that the general opinion of the profession is that it is quite an exception; that it is a rarity, but that it is a possibility.*" The remainder of the quotation is wholly incorrect, Mr. Lane using no such words as from "it is quite possible" to "cleanliness;" the final words should have been, "my knowledge does not enable me to say how unfrequent it is, nor can any *one's* knowledge enable him to *state the frequency of it.*"

Dr. Barr's answer to Q. 1,482 is as follows:—"*I mean to say that it may, in certain cases, occur that* a woman at the time is free from disease," etc. In answer to Q. 4,740 he says, "The chances are much less where the means are used to prevent contagion. I say it is to a great extent preventible by using proper care and ablutions. It is among the dirty women, who are not able to use proper ablutions and necessary safeguards, that the contagion chiefly exists. I never said that the women who were clean were capable of extensive mediate contagion. It is the dirty women who propagate the disease in that way as well as by direct contagion." I leave the reader to say if I was not perfectly correct in my statement that Mr. James Lane and Dr. Barr consider the possibility of mediate contagion as very remote. Dr. Patterson has altered my word possibility into probability.

As for latent disease being present in the female, and therefore undiscoverable on inspection, I do not think more frequent inspection necessary. I merely urged that it was not an argument against examination, but, if any argument at all, one in favour of more frequent inspection. At the latest, it will be detected at the second or third examination, whereas a woman in Liverpool or Glasgow may spread the disease for weeks or months unchecked.

Professor Henry Lee's experiment was very interesting, but the result of Hunter's celebrated experiment has been explained in a much more probable way by Ricord, who suggested that the patient from whom Hunter took the gonorrhœal discharge had a concealed urethral chancre. It is no argument against the examination of females, which has proved to be of the greatest benefit to them, and to have reduced disease considerably in all the protected districts.

Dr. Patterson urges that Inspector Anniss' figures were disputed, not because they were believed to be incorrect, but because the suppression of brothels was no part of his duty. The Select Committee appear to have been of a different opinion. Moreover, the local police did not dispute the reduction, but claimed some of the credit for themselves.

I have already shown that the English Acts do not license brothels, hence, in this respect alone, they differ from the recent laws in Paris in relation to prostitution, where each brothel paid for its license. Moreover, the women in Paris were not free to leave when they pleased; in England, a brothel-keeper detains a woman at his or her peril. There are many other differences which need not be mentioned, the French and English social systems differing in so many respects.

The table of illegitimacy is both interesting and instructive. It would have been more so had Dr. Patterson given the proportions in different parts of England and Scotland, showing the relative proportions in agricultural and town districts. It would have been seen then that illegitimacy is determined not by Contagious Diseases Acts, but by other causes, leading to concubinage instead of marriage, and consequent illegitimacy. In 1876, in Scotland, the rate of illegitimate births was 4·7 per cent of total births in the counties of Ross and Cromarty, 17·8 and 17·3 per cent in the counties of Elgin and Banff. In 14 counties in the centre of the country the rate varied from 6 to 9 per cent, and in Dumfries, Kirkcudbright, and Wigton, it was as high as 15·8, 15·9, and 15·4 respectively; and though these rates vary from year to year, these different districts have retained very nearly the same rates for many years, and are mostly agricultural districts.

As for the colonies, I could, if this paper were not already far too long, have given interesting information as to the value of the Acts in St. Helena, New Zealand, Jamaica, Barbadoes, etc., where they have all been adopted. I must be content, however, with stating that it has been found necessary to re-enforce them in Bombay, where they were repealed, and I venture to predict that they will have to be re-enforced at the Cape of Good Hope.

Returning to the 5th Fusiliers, I find that in his speech in the House of Commons, in 1870, Dr. Lyon Playfair makes the following statement, which Dr. Patterson can verify by a reference to Hansard's Reports:—"Again, the 5th Fusiliers were at Aldershot, a protected station, for the first eight months of last year (1869), and their average daily roll from these diseases was $3\frac{1}{3}$; they then went to the unprotected districts of Glasgow and Ayr, and in the last four months of the year the daily roll was $7\frac{1}{3}$, or more than double."

Dr. Patterson twits me with having told him that he was not "familiar with the facts, and the progress the investigation of the subject has made within the last few years," and adds, "as this paper is an evidence of that, Mr. Lowndes will, I am sure, pardon me." I regret I am unable to confirm Dr. Patterson's hopes, as there are still several points in his reply in which he has completely misapprehended the evidence, and has made startling inferences which the facts do not support.

Dr. Patterson asks, in reference to my statement, that the deterioration of constitution, avoided by the reduction in the frequency of disease, is an important element in apportioning the expense of working the Acts, "Where is the proof of reduction in any constitutional form of disease? The evidence by Inspector-General Lawson clearly proves that the soldier would be safer in the unsubjected districts:—Syphilis has increased, gonorrhea has not been reduced, and the ratio of diseased women is 24 per cent higher than in 1866. The only reduction—a worthless one altogether—is in soft sores, and this is not in greater proportion than that in which it was taking place before the Acts." Now, if Dr. Patterson will refer to page 451 of the evidence before the Select Committee in 1881, he will find a return, No. 6 B, by Mr. Lawson, bearing on these points, in which it is shown, that at the stations which came under the Acts, the admission for primary venereal sores (under which term are included both infecting and non-infecting sores) for the four years, 1860-63, before any restriction was imposed, were 129·8 per 1,000 men annually, and those for secondary syphilis, 40·0 per 1,000, and for the four years 1870-73, when the Acts were in full operation at all the stations to which they were applied, the admissions for primary sores were only 52·5 per 1,000, and for secondary syphilis, 20·3 per 1,000. On the other hand, the admissions for primary sores at all the stations never under the Acts, were for the period 1860-63, 116·3 per 1,000, and for secondary syphilis, 30·5 per 1,000; and for the period 1870-73, the admissions for primary sores were 86·0 per 1,000, and for secondary syphilis 27·5 per 1,000. There was thus at the stations which came under the Acts a reduction of the primary sores of 60 per cent, and of secondary syphilis of 49 per cent; while at the stations never under the Acts the primary sores were reduced 26 per cent only, and the secondary syphilis 10 per cent. It is thus clear that there has been a much greater reduction of secondary syphilis at the stations under the Acts, than at those never under them, and, consequently a proportionate amount of deterioration of constitution avoided; and further, the reduction in primary sores has not been in the non-infecting sores (or as Dr. Patterson calls them, soft sores) only, for had the infecting sores not been diminished also, the high rates of the constitutional form of the disease would have remained without alteration.

As to gonorrhœa, if Dr. Patterson will refer to page 445 of the evidence of 1881, he will find another return handed in by Mr. Lawson, in which it is shown that the admissions for this disease, at the stations which came under the Acts, were in 1860-63, 134·6 per 1,000; and in 1870-73, 100·6 per 1,000, a reduction of 25 per cent. While at the stations never under the Acts, the admissions were 113·1 in the first period, and 95 in the last, a reduction of 16 per cent only. Further, if he will refer to the Army Medical Department Report for 1880, the first whole year since Lord Cardwell's order stopping the pay of men when under treatment for primary venereal sores, or gonorrhœa (which, as we have seen, came into operation at the end of 1873) was abrogated, and the inducement to conceal disease removed:—he will find, at page 17, that in that year, at the stations under the Acts, there were 44,026 men, among whom there were 4,387 cases of gonorrhœa or 100 per 1,000; while at all the stations never under them, among 39,869 men, there were 4,543 cases of gonorrhea, being 114 per 1,000. Clearly showing that under the Acts, the ratio of 1870-73, was not exceeded; while at the stations never under them, there had been an increase of 21 per cent. Here, then, as in the case of primary sores, the repressive power of the Acts which was apparent from the earliest investigation, has ultimately reduced gonorrhœa at the stations under them considerably lower than at those where no such influence was in operation. Thus, Inspector-General Lawson's facts, and Dr. Patterson's reading of them, are in direct opposition to each other, and the readers, I fancy, will experience no difficulty in deciding which has the greater claim on their confidence. With these illustrations before them, I will leave it to be judged whether my statement that Dr. Patterson was not familiar with the facts, and the progress the investigation of the subject has made within the last few years, has not been fully borne out.

As for the navy, there was no evidence tendered before the Committee, the latter having taken already four years in inquiring as to the effects of the Acts upon the army. Dr. Patterson's belief that it would have looked still worse for the Acts, and was better withheld for the Acts' sake, is as reckless as many of his other statements. The Government Reports of the Health of the Navy tell a very different story.

Memorials in favour of the Acts were sent last year from Portsmouth and Plymouth. The inhabitants of all the places where the Acts are in force know perfectly well that there is not the slightest probability of the Acts being repealed, hence the few memorials in their favour. But Dr. Patterson's assertion that there has not been a single petition in favour of the Acts since 1873 is incorrect. The petitions in favour of repeal are mostly got up by paid agents of the Association for the Repeal of the Acts.

Dr. Patterson has quite misrepresented the suggestion of the Select Committee as to State Hospitals. They recommended "the institution, in some of the unsubjected districts, of female Lock Hospitals supported by State aid, and by such charitable contributions as may be obtained, to which entrance shall be voluntary. Unsubjected stations, in which venereal diseases are at present most prevalent among soldiers and sailors, should be selected for this purpose. The adoption of this course would afford an opportunity for testing the value of the opinion so freely expressed by the opponents of the Acts, that an adequate system of voluntary treatment would be efficacious from a hygienic point of view." Why should not such hospitals be established in Greenock, Leith, Dundee, in each of which there is no Lock hospital, and, so far as I am aware, no Lock wards? My allusion to the Glasgow Lock Hospital was simply to show that it, like other Lock hospitals, was only kept up with difficulty, and that the managers were obliged to draw money out of the bank to meet expenses, instead of adding to the invested funds, as is done in the case of better supported hospitals.

Supposing that these Acts were repealed, it would mean the closing of eight Government Lock Hospitals, containing 626 beds. That voluntary efforts would supply the deficiency is hopeless to expect, seeing that all the Lock hospitals in the United Kingdom, supported by voluntary contributions, are only five in number, containing 45 beds for males and 157 for females. Add to this the only voluntary Lock hospital supported by the Government, the Westmoreland Lock Hospital in Dublin for females, with 60 beds. Again, Lock wards in general hospitals are a comparative rarity, and are becoming more so. And yet most medical practitioners will admit that venereal diseases are not the only ones resulting from vice, and that their exclusion from general hospitals cannot be defended on logical grounds.

And supposing the Acts repealed, would vice cease and virtue flourish as Dr. Patterson appears to think? Absurd delusion. The Acts have really only a very partial affinity to the question of morals. I suppose that a man who keeps a mistress is an immoral man, as well as he who consorts with a prostitute. The repeal of the Acts will not affect this phase of inmorality one jot, nor will it hinder the seducer in his evil career. Fornication and disease are not one and the same thing. Nor even when a man resorts to prostitutes, does disease bear any relation to the amount of vicious indulgence, depending as it does on physical causes, a long or short prepuce, and a tender or tough mucous membrane. Men must be taught to be virtuous quite apart from the question of disease, or in what a false position will teachers of morality place themselves. It is virtually saying, we cannot teach man to be pure and chaste because it is their duty to be so, we must hold out, *in terrorem*, the fear of these foul and loathsome diseases; take away that fear, and we are helpless. "The social evil" is not to be stamped out by severe laws, nor can it be ignored; some medium course is necessary. Firmly believing that the Contagious Diseases Acts supply that middle course, I have for years advocated their cause, and shall continue fearlessly to do so.

In conclusion, I must express my grateful thanks to Drs. Coats and Napier for permitting me to occupy so much space, as well as my regret for the objectionable details which I have been compelled to notice. I would wish to take this opportunity of informing those readers of the *Journal*, who may wish to know it, that the Report of the Select Committee of the House of Commons, on Contagious Diseases Acts, has been published in a cheap form, and that I shall be happy to send it post free to any one, on receipt of a written application and sevenpence in stamps.

THE MERCURIAL AND NON-MERCURIAL TREATMENT OF SYPHILIS.

By ROBERT PARK, M.D.

(*Read before the Southern Medical Society, 30th November, 1882.*)

The following paper was suggested by the perusal of an article by Mr. Cadell, which appeared in a comparatively recent number of the *Edinburgh Medical Journal*. Mr. Cadell's article, in the above mentioned *Journal*, had been preceded by a remark by the same gentleman, made before one of the Edinburgh Societies, to the effect that no one with any great experience of syphilis, now thought of treating that disease with mercury!—Mr. Cadell assuming that none but those of the inner temple are possessed of the knowledge of how to treat syphilis correctly. This peculiar expression of opinion arrested my attention at the time, and almost led me to conclude that I must have missed the article, or articles, which expounded this new line of therapeutics to be followed in dealing with this persistent zymotic.

A careful conning of journals previously perused, showed, however, that I had missed nothing, and that the followers of a new praxis had carefully reserved their therapeutic principles and the results of their practice to themselves. This, to say the least of it, was, I felt, somewhat selfish, and not quite in harmony with Edinburgh traditions, which have always inclined towards being foremost in publishing the results of advanced science as bearing upon medicine. It was, I admit, with no little curiosity and interest, therefore, that I studied Mr. Cadell's paper. Possibly, also, this may account for the great disappointment I felt when I discovered that that paper disclosed no new line of treatment, and that even no data were given whereby any one, either with or without experience, might determine for himself the propriety, or otherwise, of casting aside mercury when dealing with the treatment of syphilis.

In considering the question of the treatment of syphilis, I will set aside generalities as far as possible, and address myself to the question of the value of specific drugs for its treatment, and the conditions which give the best results with their use. I take it for granted that we all thoroughly understand the necessity of impressing all our patients with the wisdom of avoiding all excesses, undue exposure, and whatever may tend to lower their hygienic condition, and this especially when they are syphilised. I presume also that every one knows that a study of the natural history of syphilis renders it probable that under favourable circumstances the disease, like most others dependent upon zymotic cause, and having a febrile reaction, tends to end in a condition of health: provided always, that none of the phenomena of the disease—the attendant symptoms, complications, and sequelæ—kill the patient or are allowed to kill the patient in the meantime. In this respect the analogy betwixt syphilis and enteric fever may be held to be perfect. Further, both diseases are liable to relapse, and the second attack is liable to be worse than the first. In the case of syphilis, however, we have medicines which can indefinitely postpone a relapse, whereas we have none in enteric fever. This being so, and until a true vital antidote is discovered for each and all of them, all we can expect our therapeutic agents to do is to alleviate, remove, or quicken the disappearance of the symptoms, destroy or heal effects, and prevent recurrence. The question is, What drug or drugs have we possessing the greatest power to do this?

First, let me criticise Mr. Cadell's remarks about the term antidote. I do not care to split straws, and so yield the point that we possess unfortunately no agent capable of tracking a vital poison throughout the nooks and crannies of the system, and neutralising and destroying it much as an acid does an alkali, forming an innocuous *tertium quid;* or as carbolic acid does a ferment, procuring its death and inactivity. But that is not the sense in which the word is used in medicine. In medicine the word is used to imply an agent which counteracts, neutralises, negatives, or destroys another agent *or its effects.* We give atropia as an antidote to morphia—not to destroy morphia in the blood—(we *know* it can't do that *in the blood* any more than *out* of it)—but to induce an opposing physiological state. We give oil and charcoal, &c., not so much to destroy the poisoning agent as to shield the organs from it and assuage its effects. We inject ammonia, administer ether or nitrite of amyl, in snake bite, not in the hope of destroying the infinitesimal, but potent instillation, but to set up an opposing physiological crisis, or to maintain the citadel of life till a vital reaction becomes established. In this way it may be quite correct, and is quite correct, to speak of our having antidotes to diphtheria, scarlet fever, or syphilis.

We are now concerned with syphilis.

The primary lesion is one of the best defined and most easily recognised of any in the whole domain of pathology. What its behaviour may be when simply protected from injury and allowed to pursue its course indefinitely is not known to me personally, as I have never found a patient who was willing to submit to the experiment. But I have had cases under treatment—many of them—which strongly incline me to believe that the tendency of the primary sore is not towards spontaneous healing, but towards enlargement of area and increase of sympathetic adenitis. Let me select two illustrative cases.

B. D., æt. 28, married, came to me with a sore on his penis, at the angle of reflexion of the rim of foreskin over the corona superiorly. I diagnosed a hard chancre. The patient persisted that such a thing was impossible, as he was married, and had had no foreign congress. I was annoyed at this, as he was an advanced medical student, and I thought ought to have known better than attempt to deceive. Under the circumstances, I thought it best not to argue with the fellow, and so simply stated that in that case, if he simply kept it clean and dressed it with a little lead and opium lotion twice daily, it would very soon heal up. He was not content with this, however, but himself had iodoform applied to it and various lotions and unguents for over two months, when he came to me again with phymosis, the sore having involved the whole dorsal aspect of the prepuce, preventing its retraction. There was now a multiple adenopathy in each groin. I asked him if he still persisted in his former statement. He replied in the affirmative, and was accordingly instructed to wash out the prepuce sedulously with a warm solution of potassium permanganate, and to come again in a few days. He returned in two days covered from head to foot with roseola. He also said he felt very much out of sorts, languid, and unable to study, whereat he was much disconcerted, as his examination was fixed for a fortnight afterwards.

I placed him at once upon calomel and opium pills—one every four hours, enjoining him to keep his bed for three days. When that period had elapsed, he returned, and the tumefaction was found to have sufficiently fallen to allow of the prepuce being retracted. The sore then presented the well known hard circumscribed ring, with a small central slough. A dressing of carbolic oil brought this away next day, leaving a clean-cut, cup-shaped, central ulcer, the circumferential hardness having meanwhile sensibly diminished in area. In a week's time the hardness was almost gone, its area diminished to the size of a threepenny piece, and the bottom of the ulcer level with the surrounding margins. By the time of his examination, the secondary eruption has so far disappeared as to be scarcely appreciable. *Pari passu* with this amelioration of visible phenomena, his general health improved vastly. He passed his examination, and, at the same time, from under my observation.

J. M'B., a gentleman, æt. 40, married, brought me a small bottle of corn solvent, and besought me to tell him of what it consisted. After informing him that it consisted of strong acetic acid, he stated that it had been used by him to destroy a small wart upon his penis, and had left a sore which he found difficulty in getting healed. When shown the sore, I detected a hard chancre. He asked if it would take long time to heal, whereupon I replied that, "if the sore had been caused in the manner which he indicated, then it would heal very easily and quickly, and by simple means." He assured me it could have been caused no other way, as he was a married man, &c. I gave him Lot. Plumbi c̄ Opio and Lawton's paper fibre lint to dress it with.

He had been about six weeks under treatment. The lotion had been changed several times, Condy's fluid, a weak Lot. Rubr., and so on, being tried in succession. The sore showed not the slightest tendency to heal, but gradually enlarged its area, until it became impossible to retract the prepuce to allow of the sore being dressed. At this stage my patient found it convenient to recollect that he had been in London three months or thereby previously, and had been indiscreet on one occasion, of which he was going to give full particulars, when I stopped him by stating that the details were of no moment. He said he was under the impression that the time which had elapsed was too great for any manifestation to appear. In reply, I suggested that in order to satisfy himself before submitting him to special treatment, he ought to take a hot bath and a glass of champagne before going to bed, which might hasten the appearance of secondary eruption. This he did, with the result of thoroughly convincing him that the sore could not have been caused in the manner which he originally suggested. Having a fine white skin, and being a strong and healthy man, the roseola showed very strikingly upon his body. He was now placed upon Plummer's pills, and had oleate of mercury rubbed into the inguinal regions, and in less than a weak the prepuce could be retracted with readiness, and the sore dressed. The sore, as in case I, was a perfectly typical one, and followed exactly the same course towards cure.

Now, these cases are facts of experience, and I fancy parallel facts must have come under the observation of Mr. Cadell, and most men in the profession. The inductions we are warranted in drawing from them are these—that the hard chancre offers an illustration of persistent perversion of natural action tending to destruction of tissue, and not to repair, and that mercury is a drug which, when introduced into the blood, causes a reversion to natural action tending to repair. Is there any other drug which possesses the same power, and is capable of inducing the same result? If there is it is unknown to me —iodide of potassium cannot do it. I might supply illustrative cases, but regard for space forbids.

The practical corollary is to exhibit mercury in whatever appears to be the best manner for the particular case, *as soon as a diagnosis of hard chancre has been made*, and your patient is satisfied. It is alike absurd and unscientific to wait till secondary symptoms appear. It is only an excuse for careless diagnosis, and is positively culpable in other cases than those parallel with these given above, where, if we did as we ought to do, and no secondary eruptions appeared in after years, we might lay ourselves open to the accusation of the unprincipled, that we had ruined our patient's constitution with mercury. Putting this consideration apart, it holds good with syphilis, as with all other diseases, that the sooner it is detected, and appropriate treatment adopted, the better it will be for the unfortunate patient.

Secondary syphilis in its many forms may properly be regarded as the immediate sequela of primary disease, only differing from the sequelæ incident to other zymotics in being inevitable instead of accidental. In my experience the symptoms are always worse where the patient has had only expectant treatment for the primary lesion. I have repeatedly endeavoured in such cases to obtain a restoration to health by means of potassium iodide, the Turkish bath, general tonics, &c., and failed. I have found a recurrence to mercury inevitable. Mercury, however, does not yield absolutely certain results in the secondary stage. Where it has been given for the primary disease, and has been continued, carefully watching its effects, for some weeks after the secondaries appear its action is almost invariably perfect—judged by the total disappearance of all symptoms. As a matter of routine, I follow up with potassium iodide, because the best authorities recommend the practice, and I believe it to be a safe and salutary one.

Where mercury has not been given primarily, more difficulty and longer time is experienced in subduing secondary symptoms. The drug is pushed till the gums are just slightly affected, and then continued, cautiously maintaining its influence, perhaps with two and then one dose daily, till weeks have passed. The symptoms meanwhile recede, and then gain ground again time after time. Iodide of potassium is tried in conjunction or alone, the dose being gradually run up to 30 or 40 grains, three times daily, if an intervening coryza does not prohibit. Then mercury in different form or differently exhibited is tried for some time, and even then a fissured tongue, or lip, or ulcered tonsil may remain to witness the difficulty of eradicating a vital poison from the blood once it has laid firm hold of the tissues fed thereby, and is as yet active. Victory, however, ensues in the end, the more speedily with than without mercury. I have often had to blame myself for a too cautious and hesitating use of it—never for a too free use of it.

In the later secondaries—the gummata of late life for instance, or the adenitis, mucous patches, throat ulcerations, or orifice fissures, etc., which crop up in the bodies of those who have had syphilis many years gone by, and have allowed themselves to fall into cachexia — mercury is much more powerful. In these cases the drug acts like a touchstone, a charm, a food. In such cases I have tried tonic and restorative treatment till I was tired, but without avail. As a *dernier ressort* mercury has been given, and with the most magical effect. As I write, two such cases are under treatment. One æt. 45, the father of eight healthy children—wife strong and healthy, and never had any abortions or miscarriages—sent for me to see his leg, which was the subject of an intense cellulitis with large central slough. His wife suggested that he had been taking too much liquor lately, which was evident. He acknowledged having hit his leg getting into a car. Next day no improvement—central slough forming. Next the latter was extending, notwithstanding I had put him on opium, and supported the œdematous leg with plaster nicely adjusted to allow of poultices to the central sore. In an aside he said, "You know, doctor, this is the result of early folly." "Nonsense," I replied, and made inquiries as to how long it was since he had the primary disorder, and about his family history since, and examined him to see if there was any other symptom which might lend support to his suspicion. There were none. I thought over his remark, however, and determined to examine the part more critically next day. There was not the smallest appearance of the central slough exfoliating. It was about the size of a half-crown. It was truly circular, however, and of an ash-grey colour. I was now satisfied that the man was right, and I sent him twelve powders containing in each eight grains of hyd. c. cret. and four of pulv. ipec. co.—one every eight hours. He had taken three powders before I saw him again, and for the first time since I had been in attendance confessed himself better locally and generally. Inspection of the sore revealed the fact that a line of demarcation had formed and exfoliation had begun, and in two days I was able to remove the slough entire; an ulcer with clean-cut circular edges being left. The powders were continued until thirty-six had been taken, and the cachexia, instead of increasing, visibly decreased under their use; appetite, strength, and good spirits returned, where were feebleness, anorexia, and bad temper.

Another case. H. G., æt. 50, bachelor, had been under treatment occasionally for three years for anorexia and general malaise. Has always attributed it to a syphilis contracted in his youth, when he was a patient of Lizars and Dunsmure; and I, finding no such indication anywhere, think his imagination has largely to do with his trouble. He informs me that he went through a prolonged course of medicine for the syphilis, and *inter alia*, that he consumed more than 1,000 Plummer's pills without his gums being affected. He is, therefore, very insusceptible to the physiological action of mercury. In spite of tonics he had been visibly deteriorating in physique. He passed from under observation for a time, and then came with a balanitis and urethral flux. He calmly assured me that he had long time discontinued intercourse with the other sex, and that these troubles had a spontaneous origin, and were due, he firmly believed, to the ancient syphilis. I did not believe him, gave him a lotion of boracic acid and a tonic. The symptoms disappeared. In about six weeks he returned with a preputial leucorrhœa again, and an indolent adenitis filling up the hollow in the left groin. The adenitis was multiple—not less than twelve glands being involved. He still assured me as before that no sexual congress had been experienced by him since his last visit, or for long time prior thereto. After careful drying with blotting paper, I found the whole mucous membrane of the prepuce and the glans studded with a roseolar eruption, in some places discrete, and in others concrete, forming plaques. This was the evident lesion underlying the preputial leucorrhœa. On pressing the under surface of the penis a small amount of leucorrhœal discharge was made to appear at the urethral orifice. The immediate conjecture was that it also depended upon a similar eruption within the tube, and that, taking all things into consideration, my patient's own idea of his trouble was the correct one, and that all his symptoms were referable to his old enemy. This was truly proved to be the case by the fact that under a course of Plummer's pills all his symptoms disappeared, and with them his general cachectic condition. The course of the adenitis was, I think, characteristic. The glands suppurated, but they did so separately. The surrounding cellular infiltration very slowly melted away under the mercury. The urethral discharge also disappeared under the influence of the drug, and this fact, coupled with others which have fallen under my observation, seems to render it somewhat probable that those cases of gonorrhœa, which John Hunter submits were cured by mercury, may have had a syphilitic pathology, and that some of those intractable cases of gleet which we all have to treat now and again may likewise be due to a similar cause.

It may be objected that this case is a proof against my thesis. The man took over 1,000 Plummer's pills and was not salivated, neither cured, as is evident by his suffering in the way described more than twenty years after the primary treatment. It is not pretended, however, that mercury will banish the disease from the blood for ever. I think it probable it might do this if, under proper precautions, its use were persevered in for a sufficient length of time, and were followed up by an appropriate course of iodide afterwards. The *time* over which the mercurial course is spread is unquestionably an element in the case. On inquiry, I find the patient only remained about seven or eight months under Drs. Lizars and Dunsmure, and that he had to visit the latter about five years after for mucous patches at the fundament, but did not remain long under treatment even then. When a man has an attack of ague in an Indian jungle or an American swamp, and is cured by quinine, but years afterwards at home has a recurrence of his malady, accompanied or not by an enlarged spleen, we do not exclaim that his constitution has been ruined by the previous treatment, and insist upon the patient taking some alternative but less potent drug. If not in that case, why in that of mercury and syphilis? Mercury, when properly used, is quite as beneficent a drug as any in the pharmacopœia. Albeit, it is frequently urged that though beneficent in other forms, it is dangerous and useless in tertiary forms; and that iodide of potassium is here the therapeutic agent, *par excellence.* I do not wish to underrate the iodide. It is a medicine we could not dispense with in the treatment of various syphilitic affections, but it is not going beyond the facts to say that it cannot do what mercury can do in any of them; its action differing in respect of quality, quantity, and rapidity of therapeusis. Cases might be cited, and authorities quoted, *ad nauseam*, in proof of this; cases which would leave no door open for question as to connection betwixt cause (mercury) and effect (rapid disappearance of symptoms); and authorities too good to be lightly set aside. My own experience has been sufficient to afford abundant illustration of my thesis; and the wider it becomes the more am I convinced that when we fail in ridding the system of syphilis either we or our patients are to blame. Either we have not given mercury a sufficiently prolonged trial, or have used it not wisely, or our patients have not given us adequate opportunity. I have never seen a patient salivated for syphilis, notwithstanding; but am acquainted with men who had the disease upwards of twenty years ago and were treated with mercury, followed with iodide of potassium—the course covering upwards of two years—and these men are now the fathers of large families, none of whom exhibit any traces of syphilis.

OBSTINATE VOMITING IN PREGNANCY.

By W. J. BROCK, M.B.Ed., F.F.P.S.G.,
Assistant Physician to the Maternity Hospital, and Assistant Dispensary Physician, Royal Infirmary, Glasgow.

I AM induced to make some remarks upon this subject of obstinate vomiting in pregnancy, from the fact that within the last five years three cases of albuminuria, during gestation, have come under my observation, in all of which untoward results occurred, and in two of these obstinate vomiting was the prominent symptom. In one of the cases, the only one of which I mean to give complete details—the others being of too short duration to warrant any lengthened comment—the history and the results of treatment are of the greatest importance in their bearing upon the much debated point of etiology.

In the first place, I shall briefly allude to the theories that have been held, and that are held at present as to the causation of this often serious malady.

The celebrated Bretonneau, of Tours (*Bulletin de Thérapeutique*, Août, 1846), considered from analogy that these vomitings were purely sympathetic, and depended upon a deficient dilatability of the uterus in proportion to the development of the ovum.

Dr. Graílly Hewitt, at a meeting of the London Obstetrical Society, April 1871, read a paper on the subject, which gave rise to an animated discussion. Dr. Hewitt was of opinion that an analogous cause must be in operation in the slight cases as in the more serious, and he concurred with the general opinion of the time that the sickness was due to the distending effects of the increasing contents of the uterus exciting in a reflex manner the act of vomiting; but this condition alone was not sufficient without flexion of the organ. He considered, then, that the existence of flexion was the prime causal factor of the vomiting of pregnancy, and that the slight cases, where the sickness was limited to the time of rising from bed, were explained by the action of gravity—the erect position suddenly bending the uterus on itself. To use his own words, "The compression of the nerves at the seat of the flexion, a compression increased and intensified by every circumstance increasing the degree of the flexion, is, I believe, the almost universal cause of the sickness of pregnancy. The tissues of the uterus resist expansion—this is the cause assigned by Dr. Tyler Smith. Unquestionably this is the case, but if I am correct in my view, this resistance is not enough apart from the conjoined flexion of the organ to account for more than a small number of cases." (At this meeting Dr. Tilt, Dr. Braxton Hicks, Dr. Playfair, and others, disagreed with him on the point.) Subsequently (*Brit. Med. Journal*, 11th August, 1880), in speaking of Copeman's treatment, he says that he considers his method successful owing to the straightening and rectifying of the uterine distortion which resulted from the operation of dilating the os.

Dr. Barnes (*Lancet*, January 1873, p. 551), says the vomiting is due to stretching of uterine muscular fibre under the eccentric pressure of the growing ovum and the turgescence of the uterine vessels. He also refers to the fact that in some cases, as pointed out by Sir James Simpson, the urine is albuminous, but that this may be a secondary result of the vomiting brought about by the poisonous condition of the blood, and that it would appear to be exceptional. Further, as stated by him, at the Obstetrical Society's Meeting, April 1871, he is of opinion that flexions are often present without vomiting, and that the most obstinate vomiting occurs when there is no flexion. "In fact, there was only one constant condition, that being stretching of the uterine fibre. Growth, that kept pace with the growth of the contents of the uterus did not cause vomiting, but it was caused whenever the fibre was stretched rapidly, the distending contents outrunning the accommodating growth of the uterus. When vomiting had once become excessive, another element entered into the case. The defective nutrition was attended by impoverishment of the blood, and the blood was further degraded by absorption of noxious materials from the system; hence a double toxæmia. Concurrently with this the habit of vomiting had induced a morbidly irritable state of the spinal cord, so that it readily responded to the slightest peripheral or emotional excitement. Thus, obstinate vomiting, like chorea, was maintained by an induced irritability of the nervous centres. Vomiting, in fact, now resembled convulsive neuroses."

Dr. Henry Bennett says (*Lancet*, January 1875), "According to the experience of my entire obstetrical career, extreme irritable sickness, during pregnancy, is generally occasioned by the accidental existence of inflammatory mischief of the uterus, or of actual chronic inflammation of the body or of the neck of that organ."

Dr. W. M. Turner (*West India Quarterly Magazine*, August 1861), in an interesting paper on the subject states, "The ultimate and prime cause of this vomiting is, in my opinion, the *pressure* exercised on the blood-vessels by the gravid uterus. Irregularity of the circulation is thus induced. We find œdema of the lower extremities proving this fact. Why should not the opposite part of the body suffer likewise from the same cause. The brain, for instance, must be improperly emptied and badly irrigated. Its function is disordered—hence the derangement of stomach through sympathetic media. This seems to me to be natural, at least, for by it can be explained all causes of morning sickness and all absence of it. Pressure on the blood-vessels I consider then the *prime cause of vomiting in pregnancy.* We know that the womb, by pressure on the vena cava descendens, does create an œdema of the lower extremities; also, we have neuralgia produced by the pressure of the same organ on the nerves, sacral plexus, &c." . . . "It is very natural that the sickness should be so decided in the morning, for the recumbent position is eminently favourable to bringing about the pressure as mentioned. The feeling of nausea passes away as the patient stirs about, and as the circulation becomes improved and more general."

I shall give one instance more of the diversity of opinion existing on the subject under discussion, and in this case shall quote a guarded, but somewhat vague exposition of the reason for the occurrence of vomiting.

Dr. Inman (*Brit. Med. Journal*, Mar. 24, 1860,) says—"Uterine sympathy does not hold so prominent a place as the formation of a new being, for other affections of the uterus never do produce these constant symptoms." The symptoms, he holds, do not occur in perfectly healthy and strong women, in country air. He ascribes the phenomena to "that most recondite of all subjects, vital power. It leads us to speculate upon the condition of such power in a woman who is beginning to impart some of her life to a new being."

I believe that in the above summary I have included all the typical views of writers upon this subject.

I have perused the reports of cases recorded by some 50 European practitioners, and, strange to say, whether or no the urine has been examined by them, scarcely a case is mentioned of its having been subjected to the test for albumen. Professor Gustave Braune, Vienna, in discussing the subject of vomiting (*Zeitsch. der K. K. Gesel. der Aerzte in Wien.*, Nos. 39 and 48, 1863) does, indeed, mention that Frerichs directed attention to the coincidence of vomiting with albuminuria and uræmia. Sir James Simpson noticed that obstinate vomiting is sometimes connected with albuminuria in the pregnant state, and

Dr. Paul Henry Stokoe, A.B., Peckham (1875), remarks that by far the most severe and protracted attacks of vomiting he had witnessed ensued from over stimulation, the sufferers losing self-control, and finding it impossible, in their intolerable drought, to refrain from incessantly imbibing huge quantities of any handy thirst quencher; and he further adds, by way of caution, that the above mentioned condition is sometimes associated with albuminous urine, even in the earlier months of pregnancy. In an account of Dr. Copeman's third case, where he found dilating of the os succeed, it is stated that the operation was effected on the 6th April, 1875, when the patient was at the eighth month, and was found to have albuminuria.

With some such exceptions, the cases are detailed without any reference to the condition of the urine. This is even so in a list of five very severe cases, of which three were fatal, recorded in the *Medical Times* of 1st August, 1863.

One of the objects in making this communication to the *Journal* is to impress upon all who have the misfortune to encounter a case of obstinate vomiting the importance of testing the urine, for from the rapidity with which the albumen sometimes came and went in the case about to be described, I judge that practitioners, from only one or two examinations in a case, may be apt to conclude that no albumen is present.

Before entering into the particulars of the principal case, allusion may be made to the other two which, although probably less interesting, are of sufficient consequence to be mentioned in connection with it.

I. Mrs. P., aged about 35, first came under my notice five years ago. She was then pregnant, and complained much of sickness throughout the whole period of gestation. I omitted to make any examination of the urine, and therefore cannot say whether albumen was present at this time. She stated that vomiting was always very severe and trying with her while carrying her children. Her confinement was normal. She again became pregnant about eight months after delivery and vomiting was very severe. The urine at this time was tested, and found albuminous. Sickness was relieved to some extent by various remedies, and she carried her child to full term; but at the time of its delivery she complained of haziness of vision and confusion of ideas. She made a good recovery, and I frequently afterwards examined the urine, and found it free of albumen. She again became pregnant in September, 1881, and complained, as before, of great sickness, which was well tolerated until about the eighth month of gestation, when it suddenly got worse, and was accompanied by severe pain along the line of the diaphragm. A convulsion occurred, and premature delivery took place within a few hours. During this pregnancy the urine was frequently examined, and was sometimes found albuminous and sometimes not. At the time of her premature confinement it was highly albuminous. Although she was apparently sensible when delivered, she cannot now remember anything of what took place at that time. Examination *per vaginam* showed the uterus to be healthy, and normal in its position. Weeks before labour set in the os was found to be patent enough to admit the tips of two fingers, and its feel was soft and dilatable.

II. Mrs. C., aged 38, became pregnant for the sixth time four and a-half years ago (two of the conceptions had been miscarried). The urine was albuminous. She aborted at the second month. She became pregnant a year afterwards, and again aborted between the second and third months. Albumen was at this time discovered in urine drawn off by the catheter. Her next pregnancy terminated normally, and at full term last May. I examined her urine several times, and generally, though not always, found albumen in it. I never discovered the least trace of albumen when she was not pregnant. Sickness, though troublesome in this case, was not so marked a condition at any time of gestation as to cause alarm. The position of the uterus was always found to be normal, and its condition healthy.

The facts in connection with these cases led me, when in attendance upon the one I am now about to detail, to examine into the condition of the urine from day to day, and often twice a day.

III. Mrs. M., aged 29. First date of attendance, 11th July, 1882.

Previous History.—Married over seven years. Menstruation prior to marriage always very regular. Had first child about six and a-half years ago, between the sixth and seventh months. It was born alive but soon died. Sickness was a constant symptom. Four months afterwards (she is positive in this assertion) she had a miscarriage of a formed child. She states that she suffered from hæmorrhagic discharges between the periods of these deliveries, so that there is at least a probability of the second fœtus being a twin arrested in its development, and not expelled when its neighbour was. She again became pregnant, and carried the child to full term, though she was very sick all the while. This child was born two years or so after the last miscarriage, and is still alive. She again had a child in fourteen months, which was born at the eighth month and lived. Constant sickness was the rule while she carried it. The next conception took place exactly one year afterwards, and so great was the vomiting that premature labour was induced before the seventh month. It was with difficulty the child was kept alive, but it survives and is now healthy looking. In ten months after this she had another miscarriage, brought on by constant vomiting, and said to be at the fourth month.

When called to see her on the 11th July, I learned from her that she menstruated regularly since her last miscarriage (December, 1881), but had now missed two periods, and had been dreadfully sick all the time, especially in the forenoons. I ascertained, however, that now the sickness was lasting all day. She was put to bed on the 13th. I examined the uterus and found it enlarged and broadened, as I anticipated, but perfectly normal in every respect.

From the time I first saw her until the 23rd the symptoms got worse and worse, despite every remedy tried, and I tried many. Beginning with the most simple, such as infusion of green tea with ice to suck, I had recourse to the following remedies throughout her illness:—Subnitrate of bismuth in thirty grain doses, and ultimately in drachm doses, given dry in rice wafers; calomel and opium in pill; oxalate of cerium in various quantities, from two up to ten grains; bromide of potassium alone and in combination with bismuth; creasote, which seemed to make matters worse; chloral by the mouth and per rectum; tincture of iodine in five to seven drop doses, which seemed to have some action, though only for two or three hours; ipecacuanha wine in drop and three drop doses; belladonna externally and by the rectum; saline enemata—these gave a sense of relief but did not diminish the sickness; nitrite of amyl by inhalation; chloroform by inhalation; chlorodyne; chloric ether; iced champagne; ice to the spine and epigastrium; sinapisms; morphia hypodermically; ingluvin. Of all these so-called remedies the only ones that seemed to be of the slightest use were tincture of iodine, which did appear to allay sickness for a little while, and the hypodermic injection of as much as half and even three-quarters of a grain of morphia. The morphia in large doses had undoubtedly a very marked effect, but relief was only obtained when she was deeply under its influence. Another application used was a cotton wool plug, soaked in glycerine and placed behind the cervix. This set up a plentiful vaginal secretion, and seemed to give slight relief. As pain, however, was soon complained of in the lumbar region I discontinued the use of the plug. The morphia had to be given up on account of the constant and increasing stupor, which, though present to a slight extent now and again before the morphia was resorted to, became alarming afterwards.

During all this period, extending now over eleven days, sleep was got only twice or thrice in the twenty-four hours, by snatches of a few minutes at a time, and anything given the patient in the form of nourishment by the mouth lay no more than two minutes on the stomach. Nutrient enemata of beef tea and milk were used from the first. I examined the urine on the 14th, and found it albuminous. It remained albuminous as long as the sickness lasted, except on three occasions when it was perfectly normal. Latterly the water, as the patient was unable to pass it, had always to be drawn off by the catheter. The quantity of albumen present was generally about one-fifth part. Heat and nitric acid were the tests employed.

The pulse soon gained in rapidity and lost in volume, until on the 23rd it could scarcely be felt and counted. As fainting fits occurred on the 21st and 22nd, I felt it was dangerous to allow the case to go further without emptying the uterus.

On the 22nd, before having recourse to such a proceeding, I called in Professor Leishman, who was quite of the opinion that Copeman's method, and, it failing, premature induction of labour should be proceeded with at once.

With perfect ease I inserted my middle finger over the first joint through the internal os, without rupturing the membranes. As this manipulation seemed to produce no beneficial result within eight hours, I passed in two fingers and kept them dilating the os for upwards of ten minutes. I then waited for other twenty-four hours, and, as I observed no signs of improvement, and as I could scarcely feel the pulse at the wrist, I resolved upon emptying the uterus without delay.

Considering the excessive weakness of the patient, I had great misgivings about the result, and provided myself with a hypodermic syringe filled with sulphuric ether—a suggestion of Dr. W. L. Reid's—to be used in case of syncope occurring. I asked my friend Dr. Renfrew to be present, and late on Sunday evening, the 23rd, I removed the ovum, happily without the slightest difficulty. After disconnecting the ovum I found it impossible to take it out entire through the os, so I ruptured the sac and then withdrew it.

After the removal sickness occurred only twice during the night and but once the following day.

Albuminuria was present on the 24th, after which date no trace of it could be found, though daily looked for during the following week.

I was never able to detect any tube casts either in the urine of this patient or in the urine of the other two referred to.

The lochial discharge disappeared in three days. The patient rapidly gained strength, and was out of bed eight days after parting with the conception. She continued well and menstruated in the usual quantity in September, October, and November, each period extending over three or four days. She again "changed" on the 17th December, but the discharge was scanty and accompanied by much nausea and vomiting, which gradually got worse, and I was sent for on the 23rd. On arrival, I found a state of matters beginning very similar to what occurred before, but I learned that a slight discharge, which appeared on the 17th and 18th and had stopped, had again taken place that morning. I examined the urine drawn off by the catheter and found it albuminous. In addition to some of the other remedies used on a former occasion, I tried dry cupping over the loins, which, however, produced no good effects. On the 25th I was just about to proceed with a hypodermic injection of pilocarpin, when the discharge recurred in greater quantity and clotted, with a fœtid smell. The sickness immediately improved, and the urine was now completely free of albumen. The fœtid discharge, however, continued until the 30th December.

There is little doubt, I think, that here conception had occurred, although the ovum was not detected in the discharge.

It is certainly not legitimate to draw *positive* conclusions from the limited experience above related, but I cannot refrain from putting forward one or two *negative* opinions which may fairly be formed from these cases.

Firstly, it is by no means quite certain that Bretonneau and Barnes are correct in their explanations of the sickness of pregnancy; for here are two cases occurring in *multiparæ*, where the uterine tissues were lax, in one of which the *very* violent retchings causing premature labour came on suddenly, and in which the os, moreover, was patent for some time previously and easily dilatable: in the other, so flaccid was the whole cervix that the middle finger was passed readily through the internal os, and, further, after dilating with two fingers, the symptoms were not relieved.

Secondly, Copeman's method of treatment is not to be relied upon.

Thirdly, Dr. Graily Hewitt's ante-flexion theory is challenged, as is also Dr. Bennett's inflammatory theory, by my failure on the closest scrutiny to detect flexion, distortion, or even the history of inflammation of any part of the uterus.

Fourthly, consideration of these cases suggests that it is not improbable that albuminuria, if carefully and continuously looked for, may be found in a greater number of cases of obstinate vomiting in pregnancy than is generally supposed.

A question in the history of this case naturally arises—Was this albuminuria primary, and if so, probably therefore partly causal to the vomiting; or was it secondary and the direct result of the vomiting; or was it a manifestation of a neurotic affection which was probably also the cause of the sickness?

In the first place, was it primary? It might not be unreasonable to suppose it primary, seeing how soon it was discovered in the second illness of Mrs. M. But it is extremely improbable that it was primary, and this for the following reasons:—(1.) Albuminuria in the non-pregnant state does not necessarily cause vomiting; (2.) albumen is frequently present in the urine in the pregnant state, and vomiting is not a more marked symptom in patients with albumen present than in those where the albumen is absent; (3.) the ordinary sickness of pregnancy does take place without a trace of albuminuria, and severe and fatal cases occur where the urine is free from albumen—if it may be inferred from reports of such cases being silent on the point that no albumen existed.

In the next place, was it secondary, and a result either of poisoning or impoverishment of the blood? As already remarked, this was Dr. Barnes' conjecture of the occurrence of albuminuria in such cases. I am led, however, to conclude it was not for the following reasons—(1.) The albumen was totally absent on several occasions during the course of the illness, and entirely disappeared immediately on the cessation of vomiting, an occurrence which was unlikely to happen if the albuminuria in the first instance was caused by poisoning or impoverishment of the blood. (2.) As stated before, severe cases, and cases fatal from exhaustion, are reported where no allusion is made to the presence of albumen in the urine. (3.) Albuminuria is frequently present in the pregnant state when there is no reason to believe there is the slightest poisoning or impoverishment of blood. (4.) Obstinate and long continued vomiting, at least in the non-pregnant state, does not necessarily produce albuminuria; for about three years ago a lady came under my care who suffered from periodical sickness, with violent retchings, which, on each occasion of attack, brought her to death's door. Professor Gairdner was called in consultation to see this patient, and he, too, as I had been, was puzzled to account for the sickness. This lady subsequently died in one of the attacks. I have since learned that previous to each attack she was drinking; but at the times I attended her there was no jaundiced condition or other objective sign to arouse my suspicion that drink caused the illness. It is true she craved for brandy and water, and perhaps for all I know got much more than the allowance prescribed. I have known this woman to vomit continuously for ten days or a fortnight until she was pulseless and unable to raise her head; still the urine was never found albuminous, though often suspected to be so from an occasional puffiness of the face and hands seen during an attack. It soon became perfectly clear that the attacks were not in any way connected with menstruation, and the only conclusion that I could come to was that the vomiting, however set up, was due to excitation of the nerve centres, and that this vomiting once fairly started became, as Dr. Barnes might describe it, a condition "resembling convulsive neuroses," and might therefore be maintained by a comparatively slight cause.

In the third place, believing it to be a condition not dependent upon or causal to the vomiting, I think it extremely probable that the albuminuria and the vomiting were due to the same changes in the nerve centres, however brought about. The disappearance of the albuminuria immediately on the cessation of vomiting is, I think, favourable to this view. Moreover, this is the only explanation I can really conceive of, after casting aside the other two.

Although I have so prominently brought forward albuminuria in this discussion, let it not be supposed that I expect it to be found in a majority of severe and fatal cases of vomiting in pregnancy. My principal reason for discussing at length the above cases is to provoke further enquiry into the subject of the occurrence or non-occurrence of albuminuria in the worst forms of the malady.

Seeing that I have endeavoured to refute the prevailing theories advanced upon obstinate vomiting in pregnancy, from my experience of these cases detailed, it may naturally be asked what explanation have I to offer for the occurrence of this vomiting? To this I reply that I lean strongly to the opinion that these obstinate cases of sickness take place quite independently of any peculiarity of the uterine tissues or distortion of the organ itself, and that it depends simply upon an idiosyncrasy in the individual.

Sickness, and at least nausea, are normal to the pregnant state at certain times, for a longer or shorter period, and can only be explained by that subtle action called reflex. A similar nausea can be artificially produced in the non-pregnant state by slight pressure on the ovary, and, as I myself have seen, actual sickness can be produced in some by the same operation. The intensity of the result apparently depends upon the neurotic constitution of the woman.

In one family teething in the children scarcely disturbs them. In another family it gives rise to the most troublesome consequences, and this apart, as far as can be made out, from any essential degree of difference in the difficulty which the teeth encounter in coming through the gums.

In one child we can more than guess that teething or some other affection, such as worms in the rectum, will produce little disturbance. In another we can almost count upon either of these conditions producing convulsions, granted we have had some experiences beforehand of how both children bear illness.

Of those who have frequently to make sea voyages, one will suffer only from nausea for a few days, another will invariably be affected by the most obstinate vomiting during the whole voyage. It is a well known fact that occasionally death from sea sickness takes place, and there are many people who dare not risk even a short sea passage.

I might instance many cases where central or peripheral excitation produces in one person little or no effect and in another the gravest results, but I think it unnecessary, and prefer to mention only one example more. But this time I shall give a case where the phenomena are seemingly of a similar neurotic nature, although referred very recently to a changed neurotic condition apparently localised. I allude in this instance to the phenomenon of cardiac murmur with exophthalmic goître (Graves' disease)—a phenomenon only explainable, at least at the present time, as a concomitant of an affection of the cervical sympathetic ganglia.*

---

* The following is an extract from Dr. Sansom's Lettsomian Lectures on "Mitral Regurgitation," as published in *Lancet* of 4th February, 1883:—"We will now assume that a systolic apex murmur is present in a patient showing signs of a *neurosis of the cervical sympathetic.* It has been fre-

A friend of mine has at the present moment a patient whose father suffers from angina pectoris, and whose deceased sister (unmarried) was the subject of exophthalmic goitre.* This patient enjoyed good health up till the period of her second pregnancy, when exophthalmos with a cardiac murmur made its appearance, and caused her great anxiety and suffering. Both the exophthalmos and murmur disappeared after labour, and the patient remained well until pregnancy again occurred, when the same symptoms returned—only more marked. They increased until labour took place, at which time they entirely disappeared. They remained away till two or three months ago, when they returned, although there is no evidence that the patient is now pregnant, and equally little evidence to lead to any explanation of the recurrence of these symptoms. Here, then, is a case where exophthalmos with a cardiac murmur, apparently stimulated in the first instance by pregnancy, can only be traced to a neurotic cause. This phenomenon I consider parallel to obstinate vomiting in the case of pregnancy. Whether in either case, or in any of those previously mentioned, what seems to be functional may yet be demonstrated as in reality an organic lesion remains to be seen in the future of science.

It is exactly to such a peculiarity in the nervous constitution as mentioned in the above cases that I consider is due the difference in character between the ordinary sickness in pregnancy and the severe and even fatal forms of it.

I do not feel called upon to account for the relief said to be obtained by Copeman's method of treatment, seeing that it signally failed in the case of Mrs. M.; although I can conceive

---

quently noticed that a murmur at the apex has existed in the subjects of exophthalmic goître (Graves' or Basedow's disease); yet on *post mortem* examination no disease at the mitral orifice has been discovered. In these cases anaemia may be present, but not of necessity. It is not causally related with the phenomena. Organic heart disease may co-exist, but such coincidence is rare. It is important to recognise, especially with regard to treatment, that in the subjects of Graves' disease mitral regurgitation occurs without valvular lesion. The record of fatal cases, in which disease of the cervical sympathetic ganglia has been actually demonstrated in Graves' disease, is now tolerably extensive. Trousseau, Cruise and M'Donell, Keith, and Shingleton Smith have recorded cases in which some of the ganglia (usually the inferior cervical) have been enlarged, atrophied, or degenerated."

* If, as I strongly suspect, angina is itself due to a neurosis of the sympathetic ganglia, the fact above mentioned is of the greatest interest and importance, for here would be one neurotic condition showing itself differently in different members of the same family. I may here mention that more than one member of this family have another peculiarity, namely, inability to write steadily, the grasping of the pen producing a condition *allied* to writer's cramp.

of such an operation influencing reflex action, something after the same fashion as violent rubbing of the point of the nose may prevent a sneeze. With regard to the general question of the efficacy of treatment as any reason for believing in one theory rather than another, it is scarcely necessary to enter into it, though I think that the good results obtained in most instances of the sickness point more directly to my view rather than any other being the correct one.

I take the view, then, that obstinate vomiting in pregnancy arises simply and purely from an idiosyncrasy in the individual.* Vomiting of course may be aggravated by other conditions present, such as undigested matters in the alimentary canal, &c.

I shall briefly now summarise my principal reasons for coming to this conclusion.

(1.) That obstinate vomiting occurs in multiparæ, where the uterine tissues are lax, and where the os is soft, easily dilatable, and even patent enough to admit the tips of two fingers. This causes me to reject the theory held by Bretonneau and Barnes.

(2.) That obstinate vomiting is absent in the majority of cases where there is a rigid state of the os, and where one would almost expect it invariably to be present, if the cause were that assigned by Dr. Barnes.

(3.) That obstinate vomiting is often absent in flexions and distortions of the uterus, and often present where there are no flexions or distortions. This would not be likely if Dr. Hewitt's theory were true.

(4.) Obstinate vomiting is often absent in inflammatory conditions of the uterus, and present when there are no inflammatory conditions. This ought not to be the case if Dr. Bennett's theory be correct.

(5.) Because I believe a parallel condition is to be seen in other affections clearly influenced by the individual's neurotic constitution; for instance, obstinate sea sickness, the occasional vomiting that takes place in pseudocyesis, the proneness to convulsions in certain children whenever ill; or, to take a specific case, the vomiting simulating the obstinate vomiting of pregnancy, in a non-pregnant woman, in whom the uterus was normal.†

(6.) Because there is no definite line to be drawn between the ordinary cases of sickness in pregnancy and the more severe cases.

---

* I use the term idiosyncrasy for want of a better to explain a condition seemingly functional, but which may in fact be organic.

† *Vide* page 207.

THE PHOTOGRAPHY OF MICROSCOPIC SECTIONS.

By JAMES WHITSON, M.D., F.F.P. and S.G.,
Surgeon to the Dispensary of Anderson's College; Late Extra Dispensary Surgeon, Glasgow Royal Infirmary.

(*With a Microphotograph.*)

For some time past photography has been made use of as an aid to surgery, not only with the view of furnishing an important accessory in teaching, but as a potent auxiliary towards the more perfect exposition of operative principles, and recently, though only in a very small degree, as a means for the clearer elucidation of histological research; and in many instances the assistance thus afforded has been of the greatest value. Accurate representations of rare and interesting cases can in this way be obtained for permanent record, where the original is invariably so faithfully maintained, that dispute on the point is rendered superfluous. A drawing may, and often has only too good reason to be challenged as to the veracity of its details, but a photograph never can belie itself, or be called in question thereon.

During the month of October, 1882, while taking charge of the Wards of my friend Dr. William Macewen, in the Royal Infirmary, a most unusual case of adeno-sarcoma of the mamma came under my care, and was successfully removed, the report of which I had the honour of reading before the Medico-Chirurgical Society of Glasgow, on 12th January, 1883, and which will shortly appear in the columns of the *Lancet.* Excellent sections of the tumour were prepared for me by Dr. Newman, pathologist to the Infirmary, and I was extremely anxious to obtain a good sketch of one or more of these; but unfortunately no one capable of undertaking the task could be found. The only man likely to accomplish it satisfactorily was engaged with Dr. Coats, and would not be at liberty for some months. I may here state, that though to the uninitiated it may appear very simple, it is in reality no easy matter to make a creditable representation with the pencil of microscopic sections, and any one wishing to acquire proficiency in their delineation must undergo no little previous training in this particular department of histology. Beginners, as a rule, in their earlier attempts, frequently give undue prominence to the minor details, while, at the same time, they altogether ignore, or glide hastily over the salient and typical points of the specimens, which for the time being happen to be placed before them.

Under these circumstances, I bethought myself that photography might, if sufficient care were bestowed on the process be conducted to a successful issue, and make as effective a contributor for the demonstration of microscopy as it has proved advantageous to the interests of surgery; and in conjunction with my friend, Mr. Schulze, set to work at once in order to see for ourselves what could be done in this matter. After a good many attempts we succeeded in obtaining some very fair negatives, transparencies of which were executed for me by Mr. Thomas Swan, of Messrs. George Mason & Company, 180 Sauchiehall Street, and shown by means of lime light as lantern slides at the meeting of the Medico-Chirurgical Society recently alluded to.* Soon afterwards I gave two similar exhibitions for the benefit of my many student friends in the Royal Infirmary and Andersonian Medical Societies. When well put on the screen the value of these transparencies as a teaching medium is self-evident, from the fact that the lecturer is able to place at once before his whole class, in a brilliant and attractive form, a capital exemplification of his subject, written, so to speak, in the largest and most legible of types. The great drawback to the use of lime-light is its expense, while the fitting up of the apparatus, which is necessarily rather bulky, takes some time, and demands a good deal of care in its subsequent management.

Mr. Swan also prepared some excellent silver prints of the tumour sections, which were shown by me in Dr. Macewen's Systematic Class, he being then engaged in the consideration of the sarcomata. It was, however, altogether out of the question to get ready a sufficient number of these for the present paper, and I therefore employed the Woodbury-Type Company, whose resources in this way are practically unlimited, to get ready several hundred copies, specimens of which will be found to accompany each number of the *Journal* for the present month.

The following account of the microscopical characters of the tumour has been kindly supplied me by Dr. Newman, Pathologist to the Infirmary:—Microscopic examination of sections taken from periphery of tumour show it to be composed of spindle-shaped cells imbedded in a homogeneous matrix, in addition to a large number of dilated acini and excretory ducts, the number of which varies, however, in different parts of the growth. The acini and excretory ducts are lined by a single layer of cylindrical epithelium, and in none of the sections examined is there any tendency shown to proliferation of the epithelial elements beyond their normal limits. Immediately surrounding the acini, but distinctly separated from them, there is a layer of large closely packed spindle-shaped cells, almost all of which contain well marked nuclei. These spindle cells are so large, that at first sight they appear like, and might easily be mistaken for, epithelial cells. Where the acini are close together, the intervening tissue is composed entirely of these large cells, arranged concentrically around them; but where the interacinous tissue is large in quantity, it is made up, unless close to the acini, where the cells are always large in size, of small round cells imbedded in a tolerably abundant homogeneous intercellular substance. Sections from parts of the tumour other than the periphery, reveal no difference in its microscopic characters.

---

* My best thanks are due to Mr. Swan for the extreme care which he bestowed on the arrangement and subsequent manipulation of the lime light, as well as for the many invaluable services which he rendered in connection with the production of the lantern slides.

The following is a description of the method (read before the Medico-Chirurgical Society of Glasgow, 12th January, 1883), which was adopted in the taking of these photo-micrographs, and I must here acknowledge my deep sense of obligation to Mr. Schulze, not only for the use of his valuable instruments, but for his indefatigable labours to secure a good result:—

The photographs were taken by means of a large microscope stand, made by Ross & Company, and arranged as now described. The axis was disposed horizontally, and the binocular body was replaced by a short wide monocular one, the end of which farthest removed from the objective carried a biconcave amplifying lens of one inch diameter. To the short body was attached a pyramidally-shaped mahogany photographic camera, carrying at the large end the focussing screen of ground glass. The object glass employed was one of Zeiss' D D, equivalent to one-sixth English focus, and giving a magnification of sixty diameters at ten inches, or of three hundred diameters at ten inches' distance when combined with the usual lowest, so called A ocular, of English opticians. The D D of Zeiss is a remarkably fine lens, having an air angle of 105°, or a numerical aperture of 0·79.

The object was illuminated by a powerful paraffin lamp, having a flat wick, one and a half inches broad, and a bull's eye lens of three inches diameter, which carried on its flat side a disc of dark blue glass, for the purpose of obtaining monochromatic blue light, as with the use of it the visual and actinic foci of the objective practically coincide. The lamp is the one designed by the Rev. W. H. Dallinger, F.R.S. It has beautiful vertical and horizontal motions for both lamp and bull's eye lens, so that the illumination can be adjusted to the greatest nicety. With the D D, and the previously mentioned amplifying lens the object on the negative is magnified one hundred and forty diameters, or about twenty thousand times superficially.

Some negatives were taken with *Zeiss'* C C, or one quarter of an inch objective, the object on the negative being magnified seventy diameters, or four thousand nine hundred times superficially.

The light having passed through the bull's eye lens, with its convex side turned towards the edge of the flame, was received by one of Ross's Kelner C oculars, which has a large field lens, and which ocular, placed in the sub-stage of the microscope, served as an achromatic condenser. The stand having been arranged as described, a characteristic portion of the object was selected, and carefully focussed. Particular attention was paid to an uniform and correct illumination of the field of view, by no means an easy matter when sunlight is not available. The shutter of the camera having been closed, the focussing screen was withdrawn, and replaced by a dark slide containing one of Wratten & Wainwright's instantaneous plates (4 × 4), and after the room had been darkened, leaving only the aforesaid paraffin lamp burning, a time was selected when all tremor had ceased, to expose the sensitised plates for ninety seconds, this having been found the correct time of exposure for the plates, apparatus, and object used.

The negatives were developed by Mr. Thomas Swan, who, as before stated, also produced the positive transparencies.

It is, of course, not to be expected that a photograph on the screen should be, by any means, so sharp and well defined as the image of the object in the microscope. For we have to consider that, supposing the lantern photograph to be three inches in diameter, and the picture on the screen eight feet in diameter, that the former is further amplified thirty-two times diametrically, or one thousand and twenty-four times superficially. In point of fact, we would then have before us the image of the object magnified four thousand four hundred and eighty diameters, or twenty million times superficially. The sensitive film of both negative and positive being a coarse one, microscopically speaking, causes also the loss of some of the finest details. Besides, a photo-micrograph never can possess the same depth of focus as the image projected on the retina, because the eye involuntarily adapts itself to the different foci while viewing an image, the result being increased penetration; but nevertheless I feel sanguine when this method comes to be better understood than it is now, that it will be universally used by histologists for producing permanent as well as faithful records of microscopical structures.

In conclusion, I may say that Mr. Schulze and I are still pursuing this subject, and we have every prospect before long of being able to exhibit much better specimens of photo-micrography than the one now placed before the numerous readers of the *Glasgow Medical Journal.*

CURRENT TOPICS.

The Next International Medical Congress.—It is announced that the eighth session of the International Medical Congress will be held in Copenhagen during the days from the 10th to the 16th August, 1884. The secretaries give this early intimation to facilitate arrangements, and to prevent the clashing of this with other appointments of a like nature.

The General Medical Council.—Dr. Andrew Fergus has been again chosen by the Queen at the recent meeting of the Privy Council to act as Crown representative for Scotland in the General Medical Council. The profession in Glasgow were honoured when to that distinguished position, five years ago, one of their number was appointed, for the first time since the formation of the General Medical Council; and they were confident, from the manner in which Dr. Fergus had discharged his duties as President of the Faculty of Physicians and Surgeons, that he would be no unworthy successor of Syme, Christison, and Lister, in the position which they in succession held in the Medical Council. From the great experience Dr. Fergus has had in all matters relating to medical legislation, his re-appointment at the present time is most opportune, as the Government have intimated through Lord Carlingford that next session will see introduced a bill that may profoundly change the whole system of medical licensing, and so modify greatly the present system of medical education. As it is in Scotland believed that Scotch interests will in the proposed changes need careful attention, it is fortunate that Dr. Fergu is to continue to occupy a position that will enable him to render effective service in a matter so important.

ANTI-VIVISECTION MEETING IN MANCHESTER—REJECTION OF THE RESOLUTIONS.—The direction of the tide of existing public opinion may perhaps be judged from the result of a meeting called on Feb. 1st, in Manchester, by the opponents of experiments on animals.

The proceedings began by the reading of several letters of apology for non-attendance, among which was the following from Mr. Lawson Tait:—

"I do not know that I can say anything more on the subject of vivisection than I have already published. I oppose the practice on a variety of grounds, but chiefly because, so far as I can see, it is useless and misleading. The British Medical Association has just taken a step which will do more in twelve months than vivisection would do in a century, if we may judge from its past history. I refer to the establishment of a committee for the collective investigation of disease. This is beginning in the right way, and the true progress of medicine will be thereby enormously hastened."

After this a resolution was proposed and seconded "That the practice of vivisection is directly opposed to the fundamental principles of justice and morality implanted by a beneficent Creator in the hearts of men."

This resolution was opposed by Professor Gamgee in a speech which must have been of the most telling description. He boldly attacked the agitators on the ground that they supported their views by grossly inaccurate statements, and by exaggerations of the amount of pain inflicted by physiologists in their experiments. He claimed for physiologists and medical men as strong a sense of humanity and sympathy with animals as that shown by any class of the community, and he repudiated the stigma of cruelty and indifference to the sufferings of animals which had been so freely cast on them. He asserted the virtual unanimity of the medical profession in favour of experiments on animals. Prof. Gamgee concluded by moving, with the alteration of one word, the resolution carried without a dissentient voice at the International Medical Congress in 1881. This resolution is in the following terms:—

"That this meeting records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable to its future progress, and accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion that, alike in the interests of men and animals, it is not desirable to restrict competent persons in the performance of such experiments."

The chairman ruled that this amendment was not relevant to the resolution. After some discussion the original motion against experiments on animals was put, when it was defeated by an immense majority, the numbers against it being as twenty to one in favour of it.

The next resolution bore that "the practice is not only immoral in itself, but necessarily tends, as is proved by professional evidence, to demoralise those who witness or take part in it." The seconder of this resolution made a speech which contained much unconscious humour. In the course of it a large illustration of an operation on a rabbit was produced from the platform and hung over the front of the chairman's table. The speaker intimated amidst laughter that he would not dine with Professor Ferrier at the same house. He asserted his belief that vivisection would not end with animals, but would extend to the poorer class of our fellow-creatures. He had the greatest possible reverence for the medical profession, but the younger school which was coming up was as unlike the old school as light and darkness.

This resolution was opposed by Mr. Alfred Stocks, M.R.C.S. He complained that the opponents of vivisection were in total ignorance of the facts, and were blinded by an unwise sentimentality.

The resolution was defeated by even a larger majority than the first.

The next resolution was to the effect that "Vivisection may be shown, on the highest medical authority, to be useless as an instrument of scientific discovery, while it has served to suggest and perpetuate grave scientific error."

This was opposed by Dr. Dreschfeld, who said, among other things, that so far from experiments having resulted in nothing, they had within the last few months placed within their grasp a knowledge of the causes of pulmonary consumption.

The resolution only obtained four supporters.

The meeting separated after cheers for Prof. Gamgee.

REVIEWS.

Manual of Gynecology. By D. BERRY HART, M.D., F.R.C.P.E., Lecturer on Midwifery and Diseases of Women, School of Medicine, Edin.; and A. H. BARBOUR, M.A., B.Sc., M.B., Assistant to the Professor of Midwifery, University of Edin. Edinburgh: Maclachlan & Stewart. 1882.

THIS manual consists of 644 pages, with nine lithographs and four hundred woodcuts. As may be inferred from its bulk, it takes up pretty fully the whole subject of the diseases peculiar to women. The first sixty-nine pages are devoted to the anatomy of the female pelvic organs, and this part of the subject is treated very thoroughly, both in the text and by diagrams and drawings from frozen sections. Then follows a chapter on the physical examination of the pelvic organs, also profusely illustrated. In speaking of the uterine sound, the authors recommend that of Sir J. Y. Simpson, which is figured with a handle partly of wood. It has always seemed to us that a handle wholly of the metal of which the rest of the instrument is composed is much to be preferred. Sooner or later the wood, by frequent wetting and drying, becomes loosened from the metal, and chinks and cracks afford dangerous lurking places for dirt.

The authors, while warning of their danger, nevertheless advise the use of sponge tents. Our experience strongly predisposes us to believe with Fritsch that, seeing the impossibility of keeping a sponge tent perfectly free from decomposition, it would be well to banish it from practice altogether. By the use of a bundle of laminaria tents, or one compound one, or with these aided by a dilator such as that of Ellinger, one can obtain access to the cavity of the uterus, sufficient for its thorough evacuation.

In speaking of the treatment of pelvic peritonitis, certain sedatives are recommended for the alleviation of pain. Amongst others Battley's solution is mentioned, and it is called "liquor morphiæ sedativus," the dose, like that of chlorodyne and laudanum, being given as 25 minims. The proper name of "Battley" is "liq. opii. sed. (Battley);" it is about 50 per cent stronger than chlorodyne or laudanum, and if 25 minims were given the sedative effect might be greater than the prescriber bargained for.

In the section treating of ovarian tumours the following paragraphs occur:—"It is better not to operate by this (abdominal) method when the tumour is small, but to wait until its size is that of a six months' or a nine months' pregnancy. A tumour of this size has displaced the small intestine from its usual position behind the anterior abdominal wall, and has stretched and thinned out somewhat the latter. The abdominal incision is therefore more easily made, and the intestines are out of harm's reach. Of course it should not be forgotten that, when the medical man is consulted, ovarian tumours have usually become abdominal and large." We do not think that this is sound advice. Quite a number of accidents may happen while an ovarian cyst is growing to the size of a nine months' pregnancy. Adhesions may form, rupture may take place, the pedicle may become twisted, or pregnancy may supervene and greatly complicate the management of the case. Again, no operator, working with care, would cut into the bowel in opening the abdomen, if it were not adherent to the anterior wall, and this, of course, would be much less likely in the case of a small than of a large cyst. We believe the advice given by Bantock, that an ovarian cyst should be removed as soon as it has been clearly diagnosed, is well founded and wise.

In the treatment of chronic cervical catarrh the application of medicaments on cotton wool is recommended to be made by forceps or on the ordinary sound. Experience in its use leads us to think that Playfair's probe is a greatly superior instrument for this purpose, especially when the application has to be made to the whole length of the canal.

In the treatment of chronic endometritis there is recommended, amongst other things, the introduction of a solid nitrate of silver point, which is to be allowed to remain in the cavity of the uterus until it melts. We have seen disastrous results from this practice, in the way of pelvic cellulitis and even peritonitis, and would hesitate to adopt it unless most other means had been tried in vain. A method of treating this disease, which we have frequently practised, and which originated with Bandl of Vienna, may be roughly described as follows:—A silver catheter, with many openings in its wall, is introduced into the uterus through a Ferguson's speculum, which is pressed firmly against the cervix. Into the speculum an ounce of a half-saturated solution of sulphate of copper is poured. This reaches the interior of the uterus through the catheter, which is alternately pushed in and withdrawn, so as to bring the fluid intimately into contact with the whole of the mucous membrane, and yet permit its free egress when contraction of the organ takes place, under the stimulation of the astringent solution. The catheter and speculum are not withdrawn until the fluid has been poured out.

In discussing differential diagnosis in connection with chronic metritis, the authors say in regard to pregnancy, "We have, under very favourable circumstances, diagnosed it at the fifth week, and the subsequent history has confirmed our diagnosis." Well, if we take a married woman in sound health, who has not menstruated for six weeks, whose uterus is found a little enlarged, whose mammæ are tender, and who is not disposed to take an early breakfast, we may come to the conclusion that it is highly probable she is pregnant. But when it comes to be a case of differential diagnosis, most gynecologists feel satisfied when they can speak to pregnancy with certainty at the end of the eleventh or twelfth week.

In the diagnosis of uterine fibroids a soft bougie is recommended, in order to reach the bottom of what may be a tortuous uterine cavity. "The length of the cavity is always increased in submucous, and generally in interstitial, but not in subserous tumours; it may measure six or eight inches. The direction of the canal is often tortuous in submucous, hence the passage of the sound is difficult, sometimes impossible. We feel that the sound goes so far and then catches on a hard projection. In such cases a soft (No. 8) bougie is very useful, as its flexibility allows it to pass the obstruction." The soft bougie has one radical defect, and that is its want of elasticity. It may go to the bottom of a cavity and quietly turn round, or turn at the point when it meets with any obstruction on the way, and the operator continues to push in the proximal end, deluded into thinking that the distal still passes onward, and it is ultimately withdrawn without his being relieved from the delusion. A fine whalebone sound is pretty free from this objection, because it is sufficiently elastic to take a curve and sufficiently firm to resist curling at the point.

In discussing the treatment of ruptured perineum, the following remarks are made in connection with the avoidance of rupture:—"In addition, the practitioner can materially help in preventing an awkward tear by the preliminary free inunction with medicated vaseline of the head, vagina, and skin of perineum, and by tucking in the anterior vaginal wall when projecting too much over the occiput." We cannot doubt that most practitioners must have been struck by the extraordinary lubricity of the vaginal mucus secreted during labour, and yet it is not oily in its nature. This mucus has a peculiarly softening effect on the tissues. We question much whether the rubbing on of any unctuous material would act better than the natural mucus as a lubricant, and we can understand that, in proportion as it is thoroughly applied, it will shed off the mucus, and to that extent hinder the natural softening of the tissues.

The following advice is given in regard to the treatment of dysmenorrhœa:—"Order a hot hip bath, or the feet to be put in mustard and water. On no account whatsoever allow alcohol to be given. If the mother has been giving whisky and water, or gin and water, at once point out the risk the patient is running. Do not give morphia, or other opiate, unless driven to it; always give it yourself and hypodermically, never by the mouth or rectum, and give no prescription for it." We have frequently known women in whose cases dysmenorrhea gave rise to extreme faintness, and with whom it was a question of taking a tablespoonful of spirits in hot water, or going home to spend the day in bed at the risk of losing their situations. Many women so situated are never tempted into tasting alcohol at any other time, and could not afford to have morphia injected by a medical man. We readily admit the peculiar danger of alcohol, and, in its place, often advise five to ten drops of "Battley," followed by large draughts of warm fluids.

Catheterisation of the ureter is described as by no means an easy operation, even after the introduction of the finger into the bladder, and Pawlik's method of feeling for the inter-ureteric ligament through the anterior vaginal wall, is adverted to. That it is not always very difficult we can vouch, as we have seen Pawlik introduce two catheters, one into each ureter, without any previous dilatation of the urethra.

There are few errors in the text, but the following are noticeable:—On page 18, one centimetre is translated "·04" inch instead of "·4," and on page 309 it is stated that "the uterus was stimulated to undergo subinvolution," where "involution" is evidently intended. The woodcuts, as already stated, are many in number and above the average in merit. There is a curious mistake in the representation of a bivalve speculum on page 111: the outer part of the screw being drawn on a plane, different from that of the inner portion.

We cannot but recommend this work to our readers; there is in it very little with which fault can be found, and a very great deal worthy of praise. This notice may be appropriately concluded by giving an extract from the chapter on polypi, as a sample of the style and practical nature of the book.

"*Treatment.*—Whenever it is necessary to dilate the cervix for diagnosis, we should have instruments ready to remove the tumour at the same sitting. The dilatation is effected by laminaria tents, or by Tait's graduated dilators. A good method is to place a laminaria tent in the cervix to start the dilatation: after six or eight hours chloroform the patient, fix the cervix with volsella, and introduce the graduated dilators in succession till the cervical canal is wide enough to admit the index finger: remove the polypus by the means to be described: wash out the uterine cavity with 1 to 60 carbolic solution.

"Small polypoidal projections are removed with the curette, as described under endometritis, followed by the application of carbolic acid.

"Mucous polypi are twisted off with the forceps, shown at fig. 268. It is advantageous to use forceps with a catch, as this keeps a steady hold of the tumour and leaves the operator's fingers free to twist the forceps round.

"In removing fibroids, we first ascertain the seat of insertion and size of the pedicle. When the tumour is small we can learn this by the fingers; when so large that we cannot get the fingers past the tumour to the pedicle, we probe round its base with the sound, or, laying hold of the tumour with forceps, endeavour to rotate it, and thus test the thickness of the pedicle. The pedicle will yield to torsion with the forceps. This is the simplest method, and should always be tried in the first instance; the forceps shown at fig. 252, or a pair of Nelaton's forceps (fig. 135), are most suitable. If this fail, divide the pedicle with curved scissors. Make traction with the forceps to render the pedicle tense; too forcible traction might produce inversion. Guarding the uterine wall with the fingers, carry in the curved scissors. In cutting, make the scissors hug the surface of the tumour, and thus keep clear of the uterine wall. To divide the pedicle Sir James Simpson introduced the polyptome (fig. 269). Strangulation by ligature, formerly widely practised, is now entirely abandoned; the sloughing stump was a fruitful source of septicæmia.

"When the pedicle is of considerable thickness, it may be divided with the écraseur or with the galvano-caustic wire. The wire écraseur is preferable to the chain écraseur, as it is more easily applied. For the nature and method of use of the écraseur, the student is referred to treatment of carcinoma of the cervix. The galvano-caustic wire has been used extensively by Byrne, of Brooklyn, whose paper on this subject should be consulted.

"When the size of the tumour makes the pedicle inaccessible, it must be diminished. This is best effected by Hegar's method; traction is made on the tumour, which is at the same time incised in a spiral manner with scissors. The tumour is thus (as it were) unwound, till finally the pedicle is reached and divided.

"Chloroform is not necessary for the removal of smaller polypi. The section of the pedicle is painless; if pain be present on tightening the écraseur round the neck of a polypus, the operator should examine carefully again to make sure that the wire is not constricting the inverted fundus. Where the polypus is large and the operation tedious, it is better to have the patient anæsthetised, as the operator has then more freedom.

The Physiology and Pathology of the Blood. By Richard Norris, M.D., F.R.S.E., Professor of Physiology, Queen's College, Birmingham. London: Smith, Elder & Co. 1882.

Note on the Formation of Fibrine, and *Dr. Norris's Third Corpuscle of the Blood: A Criticism and Refutation.* By Mrs. Ernest Hart, reprinted from *Quarterly Journal of Microscopical Science*, July 1882, and *London Medical Record*, 15th October 1882.

The name of Professor Norris is well known in connection with a number of researches, which bear the marks of originality and ingenuity, and are of much physiological interest. His memoirs on the aggregation of blood corpuscles, the extrusion of blood corpuscles, muscular irritability, and rigor mortis, prepare us to yield careful attention to the present volume. The title scarcely tells the nature of Dr. Norris's book, which is not a handbook setting forth in detail all that is known concerning the blood in health and disease, but an account of curious original researches, which have led the writer to conclusions altogether different from those which are usually held. It is beautifully illustrated, with a large number of microphotographs, and everywhere gives abundant evidence of the author's ingenuity. The only criticisms which we feel inclined to make as to the style in which, as a book, it appears are, that it would have been well if, in every instance, references had been made sufficiently precisely, that is to say, mentioning the name of the work, and the page referred to; and that it would have been convenient if the headings of the pages had given some information, instead of always repeating the unnecessary legend, "Dr. Norris on Mammalian Blood."

The subject of Dr. Norris's book is, as he tells us, a research based on the discovery of "the existence in mammalian blood of a previously unknown corpuscle, which explains the origin of the red disc, and the formation of fibrine." This corpuscle he calls by the names "invisible" and "fugitive," and describes as a disc similar to the red disc, but uncoloured, and of a refractive index the same as that of the liquor sanguinis, so that in ordinary circumstances it is invisible. He discovered it while engaged in experiments on microphotography. In some of the photographs corpuscles were observed which, although obviously in the same plane with the others, were barely visible, and were not to be seen at all in the original specimens, however carefully looked for. But what was in these cases seen under the microscope was a clear space, round which the other corpuscles were clustered, a space which was of just the size to have contained one corpuscle like themselves. Our author proceeded to seek for some plan by means of which the new structure, which he believed he had thus detected, might be separated from the liquor sanguinis which masked it, and be made the subject of further observation. One plan, which he calls "packing," is to strap a slightly convex cover to a slide, placing the convexity downwards, then, by capillary attraction, introducing the blood at one side, so that the corpuscles are arrested where the glasses are in contact, while the fluid is allowed to flow on to the other side of the convexity. In this way he gets corpuscles closely packed in a single layer, and his photographs show multitudes of clear discs prepared in this way, which come out white and circular, surrounded by red masses, which come out dark, and are compressed at their margins, so as to exhibit angles and concavities where in contact with the uncoloured discs; while another photograph of blood to which salt has been added exhibits mutual compression of dark and light discs. "Ordinarily," Dr. Norris tells us, "these (the unseen circular bodies) are more limpid than the red ones, and are indented by them;" and he appeals to photographs of blood prepared by other methods in favour of this statement, which, however; amounts to an acknowledgment that the "packing method" induces changes in the physical constitution of the corpuscles. We have no doubt that this really is the case, but are still more firmly persuaded that change is likewise induced by the other method which Dr. Norris resorts to, and terms "isolation," and which consists in making the corpuscles adhere to a glass surface, so that they may be freed from the liquor sanguinis, taking advantage of the observation, that "the adhesiveness of the blood corpuscles to foreign substances, *e.g.*, glass, was inversely as their degree" of colour. In fact, it soon emerges, that it is not simply one new corpuscle which we are asked to take to our bosoms, but a whole blooming family of different ages, the youngest of which are the invisible, while others differ in degree of colour, till at last we arrive at the oldest flushed with the strength of their hæmoglobin.

Let us say at once that we thoroughly agree with Dr. Norris that, keeping the ordinary white corpuscles out of view, the other blood corpuscles are not all alike, and we think that he has done good service in turning attention specially to that matter, and to the probable effect of length of continuance in circulation on the constitution of the corpuscles, and in illustrating the presence in blood, after manipulation, of discs of very different shades of colour and degrees of distinctness. But we are not prepared to admit the pre-existence, in the circulation, of all the invisible discs to which this book introduces us. For a reason which we shall state anon, we hesitate to deny their existence altogether; but that the shoals of them which we see photographed were in their natural condition when their portraits were taken we cannot admit. When we think how often, in preparing blood for class demonstrations, we have had occasion to watch with interest, not untinged with melancholy, the changes which, whether pure and simply imprisoned or mixed with various solutions, it has undergone at daily or hourly intervals, we do take some shame to ourselves that we have trifled over them instead of noting them down, and, like Dr. Norris, making them the subject of a more determined effort of thought. Still, we are convinced that the experience of others will bear us out in what is distinctly impressed on our own recollection, that it often occurs that a specimen of blood, consisting of a single layer of red corpuscles, all of the same tint, will after a time show much more than half of these same corpuscles grown faint in appearance, even to the extent of being only rendered just visible by the most careful adjustments of light, while it may happen that a few stand out redder than ever, but smaller and apparently spherical. This diminution of size and change of form has been attributed by Max Schultze and others to contractility, and it may be due to that cause, though the evidence seems imperfect; but what we have to do with at present is that red corpuscles, in circumstances which do not produce protrusion or crenation of their margins, behave differently one from another as regards fixation or diffusion of the coloured part of their contents; and this is distinctly favourable to Dr. Norris's view, that there are differences of constitution among the corpuscles, which may reasonably be supposed to depend upon their age, that is to say, the length of time that they have been circulating. Considering, also, the known adhesiveness of white corpuscles to solid surfaces, it is quite reasonable to suppose with Dr. Norris that of the other corpuscles the younger are the more adhesive. But this circumstance, together with the knowledge that red corpuscles will become in many instances almost invisible on standing for some time, goes quite against the idea that the generality of the so-called invisible discs have never had red contents. For in the face of the impossibility of getting blood to circulate through clean glass tubes not previously moistened with alkaline solution, how can it be expected that changes will not take place immediately in a thin layer introduced between two plates lying one against the other? And most of all, how can we be asked to believe that no change takes place in the very corpuscles which at the same time are alleged to take an active part in coagulation, when we know that momentary contact with a foreign body causes coagulation to set in?

According to Dr. Norris, his "invisible discs" are derived from the lymphatic and other blood glands; and few, we presume, have much doubt that the red corpuscles of the blood are derived from these sources. But there is much in the methods of derivation put before us which is open to remark. We are told that the function of blood glands is to produce colourless discs of two kinds, the cellular and the nuclear, and it is elaborately sought to prove that "the primary lymph corpuscle is a cell containing a nucleus, and the advanced lymph corpuscle is this nucleus set free at a certain stage of its development." It is further stated distinctly that this primary lymph corpuscle has a cell wall. The whole story seems to run thus:—That the majority of what the author calls primary lymph corpuscles set free their nuclei to become in succession advanced or nuclear lymph corpuscles, and invisible, pale, and red blood corpuscles; while the remainder enlarge and have their nuclei multiplied within them, thus giving rise to the white blood corpuscles which are alleged, by what he calls the "minor mode" of development of red corpuscles, to be set free as invisible corpuscles within the blood circulation.

For our own part, we are free to confess that, with certain safeguards, we maintain what Dr. Norris stigmatises as a stereotyped statement, at once incorrect and confusing, namely, that "the products of the blood-glands are identical with the white corpuscles of the blood." Identical they are (those of them which pass into circulatory channels), in the sense in which Britons of all ages and differences of appearance are of identical race; and we do not think that more is meant to be maintained by any one. They are, to our thinking, all to be described as amœboid corpuscles, with one or more nuclei, and differing in size. To speak of them as having a cell wall can no longer be defended when their amœboid properties are taken into account, and we own that we cannot see that they are a different race from the amœboid corpuscles found in the connective tissues, seeing that they originate in a stroma of connective tissue before passing into the vessels, and that they are capable of passing through capillary walls into connective tissue.

Most certainly we cannot admit that the coloured corpuscles are nuclei liberated from such cells. The nucleus of a frog's coloured corpuscle is a true nucleus, and the substance around it laden with hæmoglobin is similar to the substance of the mammalian coloured corpuscle; and it is difficult to understand how at the present day the theory that red corpuscles are free nuclei, promulgated by the late Professor Bennett in days when the notions of the nature of nucleated so called cells were very imperfect, can be maintained.

Dr. Bennett is also quoted to the effect that "in chyle taken from the thoracic duct there are also biconcave flattened discs, exactly resembling the coloured blood corpuscles in size and form, but destitute of colour." These are the structures which Dr. Norris calls advanced lymph corpuscles, and we do not doubt Dr. Bennett's observation, but only would have liked a more explicit statement of Dr. Norris's verification of it, and the circumstances under which the verification was made; whether in fluid taken from a thoracic duct after exposure to the air, or taken from a living animal, and in what circumstances of digestion, or of motion or rest of the limbs. It is because we believe the observation of Dr. Bennett that we are not disposed to deny that some colourless clear discs really occur in the blood circulation, though we are sure that multitudes of them, found after the blood is drawn for some time, are the result of alterations in the ordinary red corpuscles.

Dr. Norris believes that in the course of his microscopic researches he has "accumulated a considerable amount of evidence which goes to show that *the fugitive discs of the blood* produce by their degenerations and transformations the delicate networks, films, and masses, which constitute the fibrinous deposits of the blood;" and he gives photographs to illustrate that "the *colourless discs* fuse together and form pools of clear transparent *liquid*, which is immiscible in the liquor sanguinis," and also that there is "direct conversion of colourless discs into delicate fibres by the combined action of annulation and extension by currents."

In her "Note on the Formation of Fibrine," Dr. Norris's persistent critic, Mrs. Ernest Hart, speaking of the "colourless Norris corpuscle," says:—"If the glass surfaces be allowed to remain in contact for a moment, the colourless corpuscles are found to have lost their globular form and to have become pyriform and elongated." Also, "On leaving the glass surfaces still longer in contact, these pale corpuscles are observed to undergo a remarkable change. They send out long processes or tails, which bifurcate and divaricate in every direction;" and "on allowing a still longer interval to elapse, so that it is more than probable that coagulation would occur in a film of blood lying between two glass surfaces, and on separating those surfaces, perfect specimens of fibrine may be obtained after staining." Mrs. Hart's drawings in illustration of these statements are very pretty, and we have no doubt that if we had the honour to examine the preparations, we should concur with the "many critical histologists and physiologists who have expressed favourable opinions of the interest of the results" on inspecting the preparations during the last two years. On the whole, there does not seem to be any radical difference between Dr. Norris's and Mrs. Hart's view of the formation of fibrine, though undoubtedly we think Mrs. Hart's account of what she has seen, and her illustrations of this particular point, much the more satisfactory.

We demur, however, to the theory of both writers, as we understand it, that the substance or enclosed material of those pale corpuscles constitutes the fibrine of the blood. We accept it as proved that the main bulk of the coagulated fibrine is derived from the liquor sanguinis, and that only a substance of small amount, which excites the other to coagulation, flows out from the corpuscles. Not only the researches of Schmidt, but those of Lister, and the old observation of Buchanan, sufficiently show this to be the case. How else could the addition of a few corpuscles coagulate a considerable mass of fluid? The threads which Mrs. Hart depicts are probably streams of substance emerging from the corpuscles, and solidified by the reagents used.

However, considering that Mrs. Hart's views are on this matter so similar to Dr. Norris's, we venture to think she might have adopted a more friendly tone towards that gentleman. While we agree that, for the most part, the "third colourless corpuscle is," as Mrs. Hart alleges, "artificially produced," without committing ourselves by any means to the cogency of all her arguments, undoubtedly Mrs. Hart has derived from Dr. Norris an appreciation of great differences existing among the coloured corpuscles, as well as the ground work of her views, however erroneous, as to the formation of fibrin, and we are sure that her studies would lose none of their scientific importance if their results were put before the world in a form perfectly free from all acridity.

With regard to the substances thrown out by the blood discs, we take this opportunity of drawing attention to a set of researches to which, we venture to think, far too little attention has been paid, namely, those of Beale, Max Schultze, and Arndt, of which an account will be found in the memoir by Arndt in *Virchow's Archiv*, vol. 78. In these memoirs there is a full account of the remarkable power which the red corpuscles have of throwing out threads of their substance. They are excited to this by rise of temperature, and the threads separate and show vibrionic movement, and a higher temperature induces division of the corpuscles into smaller discs. Both the giving off of threads and the division of the discs we have ourselves verified, and we count these phenomena of the utmost pathological importance in connection with the evil effects of increased temperature in disease, and possibly also in connection with appearances seen within the vessels too liable to be hastily assumed to be foreign organisms.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR MACLEOD'S WARD'S.

SIX CASES OF PARTIAL EXCISION OF THE TONGUE—[Reported by Alexander Jarvie Hood, M.B., House Surgeon.]

I.—John Scott, æt. 44, clerk, admitted on 11th August, 1882. Patient was a healthy, well nourished man apparently, and never suffered from any serious illnesses till the present time. He suffered from some form of venereal disease about twenty years ago, but he does not remember the details of the ailment: at present there are hard and enlarged glands in both groins. Six weeks ago, when receiving medical advice regarding enlarged tonsils and sore throat, it was noticed that there was a small growth on the left margin of the tongue. It was quite painless, and has not increased in size since then.

On admission into hospital, it was observed that on the left margin of the tongue there was a small tumour occupying an area of the size of a shilling. It stood considerably above the level of the surrounding parts, and consisted of small separate masses, whitish in colour, firm in texture, and not deeply seated in the mass of the tongue. In the centre of it there was a clear portion, very slightly ulcerated, with, however, no discharge or offensive odour. The glands of the neck were not involved, and the inside of the mouth was unaffected.

15th August.—To-day patient was put under chloroform, and the following operation performed. Two silk threads were passed through the tongue, one on either side of the raphé, and the tongue drawn well forward. It was then split down the centre as far back as the base, and the chain of the écraseur placed round the left half, getting well beyond the seat of the disease. The écraseur was slowly tightened till the part included was separated: there was no hæmorrhage except a little oozing.

16th August.—Patient very well indeed: there has been no bleeding; taking a little milk and beef tea.

18th August.—Still progressing favourably; no pain; taking fair quantity of milk and beef tea. Mouth being washed out with solution of chlorate of potash.

25th August.—Patient is so well that he is dismissed to-day. The wound in tongue is almost healed, the discharge being almost stopped. He can articulate quite distinctly, and suffers very little inconvenience from the loss of the part of his tongue.

II.—John Adams, æt. 50, engineer, admitted 1st December, 1882.

There was nothing of importance in the family history of this patient. He was a strong, healthy looking man to all appearance, and has always enjoyed good health. In May last, a small blister appeared on the right side of his tongue, caused, he thinks, by continually smoking a short clay pipe.

The blister burst, and became very much irritated by the juice of the pipe. He consulted a medical man, who applied some form of caustic to it repeatedly, with the result, however, of it becoming more painful, harder, and larger. He was also put on constitutional treatment, but it likewise proved useless, the pain passing into the ear, and up the back of the head.

On admission, there existed on the right side of the tongue, about the centre, an ulcer about the size of a shilling, with hard, elevated, and somewhat irregular margins. A deep fissure crossed the ulcer, and there was a foul purulent discharge from its surface.

The tumour seemed to be confined to the right side of the tongue, which was considerably thickened as compared to the left side. When protruded, the tongue was drawn somewhat to the affected side. Neither the sublingual nor submaxillary glands were affected: speech not much interfered with.

6th December.—The following operation was performed to-day under chloroform. The lower lip and chin were split down to the bone, which was then divided at the symphysis by means of the chain saw. The tongue was pulled out of the mouth as before described, and split longitudinally, as far back as the base. The chain écraseur was then passed round the right half, and slowly tightened till it was removed. The cut surface was then brushed over with a solution of zinc chloride (40 grs. to ℥i.), and one vessel was ligatured. There was no hæmorrhage. The jaw was then united by means of two ivory pegs fixed into the cut surface of the bone on either side. The soft parts were then brought together with silver sutures.

10th December.—Since the operation patient has kept very well, suffering very little pain and sleeping well; taking milk, beef tea, and soda water. Discharge not profuse, and mouth being washed out with a solution, containing borax and tinc. myrrhæ. He states that the pain is now less than before the operation.

22nd December.—To-day all the stitches were removed but one: the wound in tongue almost quite healed; the jaw was quite firm, and the soft parts of chin and lip quite united. He was consequently dismissed.

III.—James Nicoll, æt. 58, gardener, admitted 4th December, 1882. This man has always been quite healthy till the present ailment began about 8 weeks ago. At that time he noticed a lump on the right side of the tongue. This gradually increased and extended to the floor of the mouth and towards the tip of the tongue.

When admitted it was observed that there was a hard firm growth involving the right side of the tongue and the floor of the mouth on the same side, and not extending beyond the middle line. It was limited behind at a point opposite the last molar tooth. The surface is slightly ulcerated and bleeds on pressure. The edges of the ulcer are hard and everted, and there is a slight discharge; hæmorrhage occasionally takes place from it. He suffers considerable pain in the tongue from time to time of a shooting character, and extending into the ear. No glands involved. He is a thin wiry man, and is a heavy smoker, preferring a short clay pipe.

6th December.—To-day patient was operated on in much the same manner as the last mentioned. The only point of difference in the various steps of the operation being, that the écraseur was placed round the half of the tongue, and also round the part of the floor of the mouth which was affected. Before applying the écraseur however, the parts involved were separated by the finger from the healthy tissues. Only one vessel, as before, required ligaturing, and the jaw was again united by means of two ivory pegs.

10th December.—Patient since operation has progressed favourably, suffering very little pain, and being able to take fair quantities of milk and beef tea. The discharge is very slight. Mouth washed out with same solution as case II.

22nd December. To-day patient was dismissed. The stitches were all taken out but one, discharge quite stopped,

21st January, 1883. Patient came up to show himself. After dismissal an abscess formed under the chin, which was opened and has closed up. Now, the wound on tongue is healed, he can speak fairly well, and has no pain in eating. The jaw is absolutely firm, and his general health is much improved, as evidenced by considerable gain in flesh and by his almost ruddy complexion.

IV.—James Sweeney, æt. 40, moulder, admitted 4th January, 1883. Patient was always a healthy man till now. Three months ago he noticed a red raw spot on right side of tongue, to which he paid no attention, as he had seen similar ones before, after heavy smoking with a short clay pipe. This one persisting, he consulted a medical man who applied caustic to it, but he failed to relieve him, and in fact it got much worse soon afterwards. For the last two months he has suffered from darting pain in the right ear.

On admission, on right side of tongue, nearly as far back as a point corresponding to the angle of the jaw, there was an ulcer about the size of a shilling. The edges were elevated and the base hard; there was very little discharge. It was entirely confined to the right side of tongue. He suffered from a severe abiding pain in the ulcer, and the shooting pains still come and go in the ear. There is slight deafness on affected side, and patient cannot speak distinctly; no glands affected. He looks a strong healthy man.

10th January.—The right half of tongue was removed to-day by the method before described. In this case also the jaw was pegged with ivory pegs.

16th January.—Since operation no unfavourable symptoms have appeared. He has suffered very little pain, and there has been no bleeding. Taking beef tea, milk, and soup in large quantities. The discharge is very little, and mouth kept clean by washing it out. Some slight swelling appeared under the jaws for a few days following operation, but this has quite gone.

22nd January.—The wound in tongue is practically healed; the jaw is firmly united, and the wound in soft part of chin is now whole. Patient can eat fairly well, and speaks quite articulately enough to be understood. Dismissed.

V.—J. N., æt. 48, admitted on 27th October, 1882.—He appeared a strong healthy man, and states that he was always a robust man. Six months before admission he felt a small hard lump below his tongue. It was not painful, but increased gradually in size for about four months, and then the medical attendant applied caustic to it, with no good effect. A month afterwards the growth was cut off with a pair of scissors, as close down to the floor of the mouth as possible. It again began to grow, and more rapidly than before, and he came in for the complete removal of it.

On admission, underneath the tongue, and binding it down to the floor of the mouth from near the tip back to the frænum, was a hard, firm tumour, about the size of a hazel nut. It was firmly adherent to the floor of the mouth, and was ulcerated on the surface towards the right side and discharged a small quantity of fœtid pus. He suffered very little pain, but experienced great inconvenience in speaking or eating. No glands were involved.

The tumour was removed to-day under chloroform as follows:—After splitting the chin and jaw, it was separated from the floor of the mouth and the under surface of the tongue sufficiently to allow of the chain écraseur being placed around it; it was then slowly removed. The other steps of the operation were the same as those detailed in the previous cases, with the exception of the way in which the jaw was brought together,—viz., by means of thick silver wire, instead of ivory pegs. The actual cautery was applied to the part from which the tumour was removed, and there was no bleeding; a drainage tube was passed through from the mouth out under the chin.

3rd November.—Little discharge from mouth, which is washed out frequently with solution of chlorate of potash. Drainage tube removed. Suffering no pain.

11th November.—Two days ago the sutures were removed from the jaw, which was tolerably firm, and to-day the sutures were removed from the soft parts. The wound in mouth is quite healed, and his speech is quite good; he was dismissed.

11th February, 1883.—Patient came up to-day to show himself. The jaw was absolutely firm and there was no return of the disease. His general health is much improved.

VI.—J. C., æt. 57, mason, admitted 23rd January, 1883.

This patient did not appear a healthy man, being haggard and debauched in appearance. He stated, however, that he was always a healthy man, his only ailment till the present being a liability to "fits," which, from his description, seem to have been epileptiform in character, and from which he has suffered for two or three years. He has been a very heavy drinker and smoker, and chewed tobacco excessively.

About July, last year, he began to experience a feeling of tenderness at the fore part of tongue when taking hot food or whisky. He did not pay much heed to this till he felt a hard mass below his tongue, but when this appeared he does not know. Soon after this he came up for advice and treatment.

On admission, the tongue was bound down to floor of mouth by a hard tumour, rather less than a walnut in size. This tumour appears to be confined to the right side, extending from the symphysis menti backwards, along the floor of mouth to the tongue, which it implicates. The tongue cannot be protruded, and the surface of the tumour is ulcerated, discharging to a slight extent. The ulcer itself is painless, but patient suffers from a darting pain in the ear of same side. Speech not much affected: no glands involved.

31st January.—Patient was chloroformed to-day, and the diseased parts removed. The lip, chin, and jaw, were split as before, and the tongue, with the tumour, separated up from the floor of the mouth, well back beyond the limits of the disease. The écraseur was placed round the anterior half of the tongue with the tumour attached to it, and those parts slowly removed.

The bleeding points were ligatured with silk, and the parts replaced as before mentioned; ivory pegs being used for the jaw.

3rd February.—Soon after the operation, hæmorrhage took place, and it was arrested with difficulty by pressure in floor of mouth and ice.

It recurred yesterday, and patient was put under chloroform, and his mouth examined. It was seen that a large clot was obscuring the bleeding vessel, and when this was removed, the bleeding at once ceased, and has not returned. Nutritive enemata were given every four hours; to-day, patient is exceedingly well. He now takes large quantities of beef tea and milk, with ℥iv of brandy.

8th February.—Patient is progressing very favourably. The jaw is almost quite firm, notwithstanding that the two fragments were again separated on the 2nd, to get at the bleeding vessel. The stump of the tongue is nearly healed, the discharge being very slight. He has no pain, and can speak fairly well. He is still a patient in the house.

MEETINGS OF SOCIETIES.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

SESSION 1882–83.

MEETING III.—TUESDAY, 12TH DEC., 1882.

In the absence of the President, MR. H. E. CLARK *occupied the Chair.*

MR. H. E. CLARK showed a CONGENITAL SARCOMA OF THE ABDOMEN. The child from whom it was removed *post-mortem* was at the time of death about a year old. The tumour had first been noticed immediately after birth, as a slight swelling in the neighbourhood of the femoral ring, and had then all the appearance of a rupture, but was irreducible. When the child was between two and three months old a swelling appeared in the inguinal canal and passed gradually down towards the testicle; it had at the canal a diameter of about half an inch and for some time it did not involve the testicle; its appearance caused the recession of the tumour in the femoral region. which now completely disappeared. The growth was not very rapid, and the child's health did not suffer till within two months of the fatal issue; indeed, the parents often congratulated themselves on the possession of a fine and healthy child. About two months before the child died the abdomen began to enlarge, tortuous veins made their appearance in the abdominal wall and rapidly extended upwards to the umbilicus, ultimately reaching as far as the chin. The child began to lose flesh and to suffer from gripes and diarrhœa, and when twelve months old it died. The *post-mortem* examination was made by Dr. Lindsay of Lesmahagow, under whose care the child had been throughout. He found the tumour occupying the greater portion of the abdominal cavity and involving the whole of the spleen, it also passed down into the scrotum and involved the testicle. It was free in the abdomen excepting at the back. where it was attached by means of numerous vessels to the mesentery, above, where it was adherent to the transverse colon, and below, where it was firmly adherent to the bladder. All the organs except the spleen and testicle appeared to be healthy, but there were evidences of a slight amount of peritonitis. After being hardened in spirit the tumour was submitted to microscopic examination by Dr. Newman, who failed to find any traces of splenic or testicular structure in the portions involving these organs; the whole tumour was composed of small round cells in a homogeneous matrix, the cells entirely agreeing with those distinguishing the *round-celled sarcoma.*

A search through the literature of the subject has only brought to light a record of one or two examples of the same nature.

MR. H. E. CLARK showed a SCIRRHUS OF THE TESTICLE AND SPERMATIC CORD. The tumour was of two years' duration. was hard and nodular, and had in front of its lower portion a thick-walled vaginal hydrocele. In its removal it was found necessary to split up the aponeurosis of the external oblique muscle, and to follow the cord upwards as far as the internal abdominal ring. The operation was performed with antiseptic precautions, and the patient made a good recovery; when last seen the cicatrix was firm and healthy looking, and the man was in good health. So rare is this form of tumour that many pathologists and surgeons deny its existence (Rindfleisch and Walshe for example), but Curling has recorded cases observed by Bryant, Nepveu, and himself, which set its existence beyond doubt, and the instance now recorded is valuable as confirming his opinion. It is also noteworthy that the extension of the disease along the spermatic cord and into the inguinal canal was a marked feature in this case as well as in those reported by Curling.

Dr. Newman showed microscopic sections of both tumours and remarked in connection with the first specimen that it is not very uncommon to find voluminous tumour in the fœtus or soon after birth, but usually these growths are composed of more highly developed tissue than in the case shown by Mr. Clark. Frequently several tissues are represented in various stages of development, so that in one tumour we may see muscle, cartilage, bone, epithelium, embryonal tissue, &c. To this class of tumours Virchow has given the name Teratoma. Cases of simple sarcomata in the fœtus or young child are very rare, only three cases having been recorded, one by Charbon and Ledeganck (sarcoma of the face), a second by Dawson (sarcoma of thigh), and a third case is published in *Virchow's Archiv.* Mr. Clark's case of scirrhus of the testicle is also of interest. It is undoubtedly a rare disease; many of those published as scirrhus of the testicle, with insufficient histological descriptions and with points of resemblance to sarcomata rather than scirrhus, must be left out of account, and only those which have been carefully examined microscopically and described accepted. Mr. Clark's case is undoubtedly one of scirrhus, and from the microscopic specimens on the table the structure may be seen to be characteristic.

PAPILLOMA OF LARYNX—REMOVAL BY PARTIAL THYROTOMY. —Drs. Coats and Knox showed a papilloma removed from the larynx of a man aged 41. It was about the size of a large bean, and had been growing for 13 years, from the under surface of the left vocal cord. It caused complete loss of voice, and owing to its extensive attachments several attempts which were made a year ago to remove it by forceps from above were only partially successful. The tumour rapidly recurred. Still, the relief granted, though temporary, was so considerable that the patient was anxious that something more should be done to restore his voice. Dr. Knox accordingly, having first performed tracheotomy and inserted a tampon, cut through the cricoid cartilage, the crico-thyroid membrane, and the lower two-thirds of the thyroid cartilage. These structures being held apart by spatulae, the tumour readily came into view, and was carefully removed with curved scissors. The vocal cords were uninjured, and as the upper part of the thyroid cartilage was undivided, they were not in any way displaced. After the operation the parts fell readily back into apposition, no deep stitches being required. A tracheotomy tube was worn for three days till the inflammatory swelling subsided. In four weeks the patient was perfectly well, and his voice was nearly restored. Dr. Coats examined him with the laryngoscope, and reported that all the movements of the larnyx were perfect. The cords were still red and congested, but moved into the middle line quite in the normal way during vocalization.

EPITHELIOMA OF LARNYX.—Dr. Knox also showed the larnyx of a man aged 26, affected with epithelioma. The disease had been rapid in its progress, and the patient had suffered so much from dyspnœa that he wore a tracheotomy tube for some months' time before death. The interior of the larynx and upper part of the trachea were nearly filled with the new growth, the vocal cords and posterior part of cricoid were destroyed by ulceration, and an opening had formed into the œsophagus.

DR. CRAWFORD RENTON showed a MYOMA OF THE UTERUS weighing 6½ lbs., which he had removed by abdominal section from a patient aged 35. The tumour was attached by a pedicle to the uterus, and this pedicle was cut through with an ordinary bistoury, the bleeding being controlled by a Foulis' tourniquet. Keith's silk ligatures were passed through the pedicle and tied, the cautery being thereafter applied and the wound closed. On the morning after the operation the patient was much collapsed, and was evidently suffering from hæmorrhage, so that, with the approval of Drs. Beatson and Allan, ether was administered and the wound opened up; the pedicle being found bleeding, it was drawn out, Kœberle's serre-nœud was applied round it and retained internally by means of a pin passed across the upper portion above the wire of the serre-nœud. Stitches were re-applied, and the patient progressed favourably, the clamp separating on the 28th day.

DR. M'VAIL showed a PERFORATING ULCER OF THE CŒCUM.

GLASGOW SOUTHERN MEDICAL SOCIETY.

MEETING III.—SESSION 1882-83.

DR. BARRAS, *President, in the Chair.*

DR. PARK read a paper on THE TREATMENT OF SYPHILIS, which appears at page 189.

Dr. Pollok said he agreed with Dr. Park in the valuable results often obtained with mercury in syphilis. It was a fact, however, that mercury produced the same disastrous effects on the system that syphilis did, and cases had frequently come under his observation in which the patient had continued on mercury far too long, and ulcerations of throat and buccal membrane had resulted. The patient attributed this ulceration to syphilis, and was treated with more mercury. This was particularly the case with the long continued use of "blood mixtures" so-called. Such cases rapidly yielded to iodide of potassium and chlorate of potash. It was also, he said, well to bear in mind the observations of Dr. Harley, who showed that mercury was a "heart poison," for the heart of a frog suspended in a solution of the bichloride ceased to beat much sooner than if suspended in pure water. It would also, he thought, be unwise to treat adynamic syphilitic patients or the strumous with mercury, for the continued administration of this drug did diminish the solid constituents of the blood, including albumen, fibrine, and the red corpuscles.

Dr. Carr thought Dr. Park's paper a most valuable one, and one that defied criticism.

Mr. Maylard said, in the treatment of syphilis the important point was to get the patient into a good hygienic condition. He considered sarsaparilla useless, except as a vehicle for the administration of more potent drugs. The acne produced by iodide was best cured by adding arsenic to the mixture. Leading syphilographers commence with mercury to touch the gums, then one-half the original dose, and subsequently one-third for eighteen months; he stated that a good formula was two grains of calomel, one-half grain of dried sulphate of iron, and one-quarter grain of opium. He corroborated the final words of wisdom of Dr. Park.

Dr. R. D. Taylor admitted the value of mercury in syphilis, but thought the dose of mercury with chalk (eight grains) too large. He did not agree with Dr. Park in allowing the patient to diagnose his disease, but having convinced himself of the existence of the disease, treated it accordingly.

Dr. Tindal agreed with the most of what Dr. Park had said. Mercury was useful, but would not depend altogether on that drug. He recommended the tonic treatment alluded to by Mr. Maylard. Coryza was never, in his experience, caused by iodide of potassium in a syphilitic patient, and he considered that the iodide was useful for diagnostic purposes, for if coryza was produced by it, he concluded that the patient was not a syphilitic one. He would not give mercury in strumous or renal cases. Prolonged use of one preparation lost its effect, so that he recommended a change from one preparation to another. Sarsaparilla, he thought, possessed no virtues.

Dr. Barras said that he had at times great difficulty with ulcers, but as soon as he put the patient on the use of the "black wash," the ulcers healed wonderfully quickly. These ulcerations he considered were syphilitic, and the rapid improvement was no doubt due to both the local and constitutional effect from absorption of the mercury. Coryza, he said, was due to the small dose of the iodide of potassium: large doses produced no such effect. He did not agree with some members in their fear of mercury in struma, and thought that Dr. Park had "thrown the wet blanket" over Dr. Cadell's theories.

Dr. Park, in reply, thanked the Society for the reception his paper had met with. He took for granted that in struma every one would combine the mercurial treatment with a tonic one. With regard to Dr. Pollok's criticisms on the physiological action of mercury, he said that no doubt mercury was a "heart poison," but so was syphilis. The line of treatment recommended by Dr. Maylard was much the same as he had adopted for the last four or five years. He assured the denouncers of sarsaparilla that that drug was a most valuable aid in many syphilitic cases. Syphilitic patients, he assured Dr. Tindal, were iodised, and coryza produced by the administration of iodide of potassium. His reasons for not treating the patients until they recognised the fact that they were suffering from syphilis were important—viz., that they might not blame him for administering mercury unnecessarily, and that they might continue it until he considered them cured.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Corrosive Sublimate as an Antiseptic Surgical Dressing. *(Cbl. f. Chir., 29, 1882, Beilage).*—Dr. Kümmell, of Hamburg, has for some time used corrosive sublimate in surgical dressings with excellent results, basing his practice on Koch's recommendation of that substance as a most powerful antiseptic, even in very dilute solutions. As disinfectant fluids simple solutions (as 1 : 1000) have been used in large quantities, and so far indications of the action of the mercury on the system (very transient salivation), have been observed in only two debilitated patients. Sublimate solutions, like 1 : 20 solution of carbolic acid, make the hands rough and harsh, but without producing tingling or anaesthesia.

The carbolic solution is used only for the spray, and to disinfect those instruments which are attacked and blunted by the sublimate. Sponges and compresses lie constantly in a 0·1 per cent sublimate solution, and all the dressing materials are prepared with the same substance. Sublimate *silk* is prepared by boiling the raw material for two hours in a 1 per cent solution; it is then kept in a 0·1 per cent solution. An excellent sublimate *catgut* also is prepared by placing the catgut for twelve hours in a 1 per cent watery solution, rolling it tightly up and preserving it in a 0·25 per cent spirituous solution of sublimate to which 10 per cent of glycerine is added.

Sublimate *cotton* and sublimate *gauze* are prepared as directed below.

To provide a dressing which might be disinfected by means of heat or the mineral acids, and rendered antiseptic by corrosive sublimate, which should readily absorb wound secretions and have the additional virtue of cheapness, inorganic materials have been used, namely, sand, ashes, and glass wool.

Ordinary white quartz *sand* is employed; it is thoroughly heated in a crucible and prepared as directed below. This is applied after the fashion of iodoform as an antiseptic powder. Thus, wound cavities may be filled with it, and covered in with a few layers of sublimate gauze and a bandage; or the wound may be stitched, drained with the capillary glass drains, covered with the glass wool and a thick sprinkling of the sand, the gauze and bandage. This dressing is applicable in cases of operation wounds about the extremities and head, abscesses, fistulæ, &c.

When the glass and sand dressing cannot be conveniently applied the *ash cushion* is useful. Ordinary coal ash is prepared as directed below, and enclosed in thin cotton bags. The cushions are made in five sizes, from 12 to 40 square ctm. They sit closely on even the most irregular surfaces, and exercise slight compression.

The glass wool is prepared as directed below. It is easily cleaned and disinfected by means of concentrated acids, and its absorbent power, due to its action as a surface drain, confers on it many advantages over the ordinary protective.

The glass wool (or glass silk) plaited in various thicknesses, forms excellent drains. Its fibres are exceedingly slender, and take up so little room in the tissues as never to lead to the formation of a drain canal; union takes place immediately they are removed; their capillary action is continuous, and is not interrupted by any kind of dressing (sand, gauze, &c.), and will bring discharge to the surface even vertically from the bottom of a cavity. In certain situations, where counter-incisions cannot be made, they are invaluable—as after removal of large cervical tumours which reach down behind the sternum, and in the drainage of Douglas's space. Nevertheless, they are suited only for aseptic wounds; for purulent fluids the old drainage tubes are necessary.

Inorganic dressings are easily used. The glass drain is inserted, a thin layer of glass wool is placed over the wound, then one or two small slightly damped ash cushions, and over all a large ash cushion, kept in place by a tolerably firm gauze bandage. The first dressing is retained till the wound is presumably healed; in about seven to ten days, in small wounds earlier, the dressing is changed and the glass drain at once removed, and a further dressing of sand or a cushion at once applied; this last dressing remains till the wound is definitively closed.

Pyrexia is usually unknown in such cases, and "aseptic wound fever" is more rarely observed than with any other antiseptic dressing; union by first intention occurs with a certainty and regularity unheard of under even the strictest Listerian treatment; drain-fistulæ have not yet been met with, nor has eczema of the skin, from this dressing and treatment.

Kümmell has obtained union by first intention in very short periods in this way: in thirteen days in an amputation in the thigh, five to eight days in herniotomies, in fourteen days in smaller necrotomies.

The materials named above are obtainable everywhere, and are very cheap. The sublimate for 100 litres of the strong solution costs about sevenpence; the ætherial solution of sublimate used to prepare 10 kilogrammes of sand, costs about the same sum; the preparation of 25 kilogrammes of sublimate ash costs rather more than a halfpenny; the materials needed for two dressings of a typical thigh amputation, including bandages and sublimate solution, cost less than a shilling, for herniotomies, about fivepence.

Since the introduction of this method of dressing in that part of the Hamburg Hospital where it is employed, no disease due to wound infection has attacked a patient so treated; formerly such diseases were there "frightfully" frequent.

The two points the author insists on in carrying out this treatment are: strict primary antisepsis, down even to the minutest details, and the retention of the first dressing as long as possible, till the wound is presumably healed.

Directions for the preparation of the inorganic dressings:—

The Ash Cushions.

1. The sewed bags are washed with green soap and soda, rinsed first in clean water, and then in solution of corrosive sublimate (1 : 1000), dried, and packed away in suitable boxes.

2. About one day before they are filled, these bags are soaked in solution No. I, wrung out, and hung up to dry on a rope, which also previously had been washed in sublimate solution.

3. 10 kilogrammes of the ashes, carefully passed through a sieve, are weighed out and gradually added, with constant stirring, to one litre of sublimate solution No. I.

4. The bags, filled with ashes to the prescribed thickness, are stitched up with thread soaked in sublimate solution (1 : 1000). The large sized cushions, Nos. i, ii, and iii, are not stitched across; No. iv has one cross row of stitches; No. v two such rows crossing each other, while No. IV has several such rows. The prepared cushions are then stored away in a close-fitting tin box.

Sublimate Sand.

5. 10 kilogrammes of sand, thoroughly roasted and passed through a fine sieve, are slowly stirred up in 100 grammes of an ætherial solution of corrosive sublimate, No. IV (10·0 sublimate : 100·0 ether).

Sublimate Gauze and Cotton.

6. Cotton wool (as prepared for dressings) is soaked in the sublimate solution, No. II, in an enamelled iron vessel, passed through a wringing machine, wrapped up in rolls, and dried for several hours in the air, or in a hot air chamber.

7. The gauze is prepared in a similar way, but directly after the wringing out is cut up in pieces of six metres length, and placed in layers in a tin box.

Glass Wool and Glass Drains.

8. The glass drains are made of four sizes, and are plaited in three strands; these, as well as the glass wool, are kept in a 1 per cent sublimate solution.

9. Before beginning the preparations of these dressings, the glass table, and every other vessel to be used, must be washed out with sublimate solution No. III.

No. I.

Sublimate, . . .	25·0
Aq. dest., . . .	4475·0
Glycerine, . . .	500·0

No. II.

Sublimate, . . .	10·0
Spt. Rect., . . .	4490·0
Glycerine, . . .	500·0

No. III.

Sublimate, . . .	5·0
Aq. dest., ...	5000·0

No. IV.

Sublimate, . . .	100·0
Æther. sulph., . .	1000·0

The Advantages of a Dry Local Treatment in Otorrhœal Diseases.—One of the greatest hindrances to cure in an ear disease accompanied by otorrhœa, whether the disease be due to inflammation in the auditory canal or middle ear, is the presence of granulations and polypoid growths. Yet one of the oldest forms of treatment of otorrhœal disease has been by copious syringing and instillation of various fluid medicines. Hence, in such treatment of this class of aural diseases, moisture has been repeatedly applied to, and kept in the ear, a naturally heated locality. Now, as heat and moisture tend to promote granulations and keep up a discharge, it is very apparent that a moist treatment of otorrhœa in many instances has a tendency to keep up rather than to check the morbid discharge from the ear.

On these grounds, therefore, Dr. Chas. H. Burnett, in a paper with the above title, in the *American Journal of the Medical Sciences* for January, 1883, holds that the syringe and all forms of drops should be omitted from the home treatment by the patient in cases of otorrhœa. The most the patient should be directed to do is to dry his ear according to its need, by running into the canal and down to the fundus a twisted pencil of absorbent cotton. The surgeon is to use the syringe only when it is absolutely necessary to remove by it the matter from the ear, and thus prepare the organ for the application of medication by his hand. This latter part of the treatment should consist in the blowing of powders into the ear. Of these, Dr. Burnett recommends one prepared by triturating equal parts of tincture of *Calendula officinalis* with boracic acid (gr. to minim), allowing evaporation, then rubbing one part of the thus calendulated boracic acid with one or two parts of pure boracic acid. Alum should not be used, on account of its tendency to produce furuncles. Comparative tables are given, which show that by the dry method of treatment the average duration of treatment may be shortened from 212 days under the old plan, to 34 days by the dry method.

Treatment of Nausea and Vomiting in Uterine Affections.—Dr. Chéron recommends the following:—

No. I.

℞.	Potass. bicarb., . . .	gr. 30.	
	Potass. bromid., . . .	gr. 30.	
	Aq., ...	℥ij.	M.

No. II.

℞.	Acid. citric., ...	gr. 60.	
	Syrup. simpl., . . .	℥i.	
	Aq., ...	℥iv.	M.

A teaspoonful of number 1 to be mixed with a tablespoonful of number 2, and the mixture to be taken during effervescence; the dose to be repeated every hour or half hour. The above represents the maximum quantity to be taken in a day. (*Revue des Maladies des Femmes*). *La France Médicale.* 18th October, 1881.—G.S.M.

Resection of the Sternum.—This operation was performed by König (Göttingen) upon a woman, aged 36 years, suffering from a tumour (osteoid chondroma) of the sternum of 2½ years' growth, but otherwise in good health. The tumour was oval in shape, situated in the body of the sternum, and extended from the level of the second costal cartilage above, to near the xyphoid process below, but did not reach much beyond the normal limits of the bone on either side. The soft parts were reflected by a crucial incision so as to expose the tumour, the manubrium, xyphoid cartilage, and inner portions of the costal cartilages. Then, by means of an elevator, the ribs were separated from the underlying soft parts about an inch and a quarter from their insertion, to allow of the introduction of a broad flat hook, against which the ribs were divided with a knife. The sternum was divided in a similar manner, at the level of the first intercostal space. Sharp hooks were then used to raise the right side of the sternum and portions of ribs still attached to it, and the bone and tumour, which projected considerably into the mediastinum, were separated from the soft parts with the finger. The same thing having been done on the left side, the upper part of the sternum was drawn forward, and the separation with the finger proceeded with from above downwards. Both mammary arteries being exposed were ligatured and divided. In spite of all care taken, the right pleura was torn at the level of the fourth rib, the air entering with a "lapping" noise, but a compress of gauze was immediately applied over the rent and held firmly there. It was then found that there was a firm adhesion between the tumour and the pericardium, necessitating the division of the sac with scissors, so that the surface of the heart was exposed. During the further separation of the tumour, the left pleura was also torn. As the operation proceeded, several assistants, following the operator's hands, promptly covered the exposed tissues with gauze compresses, which they held firmly applied on all sides.

The wound was closed with sutures, two drainage tubes inserted, and a very large antiseptic dressing applied. The patient lost a considerable quantity of blood during the long operation —two and a half hours—but made a good recovery. For the first day respiration was very rapid, and on the fourth, the pulse rate increased considerably, but the former symptoms passed off, the latter was removed by a few doses of digitalis, and no other threatening symptoms arose. The wound healed rapidly and well. The operation was performed upon 17th July last.—*Centrabl. f. Chir.*, 1882, No. 42.—D. M'P.

New Operation for the Cure of Uterine Displacements.—The following notes from a paper in the *Liverpool Medico-Chirurgical Journal* may prove interesting as demonstrating the efficacy of the operation for *prolapsus* and *retroflexio uteri*, which was simultaneously brought before the medical profession by Dr. Alexander of Liverpool and Dr. James A. Adams of this city.

The operation consists in cutting down upon the external inguinal rings where the round ligaments may be grasped, and gently drawing out these ligaments, so remedying any prolapsed or retroflexed condition the womb may be found in.

Dr. Alexander has now operated in ten cases of displacements of the womb, and he gives the results in the January number of the above mentioned *Journal*.

With one exception the cases all did remarkably well, and the women were relieved from the painful symptoms which their disease had given rise to. In one case the patient suffered from distinct retroversion of the uterus, and was also addicted to epileptic fits of a severe character. A month after the operation she left the hospital cured, not only of her uterine affection, but also of her epileptic fits. She was subject, previous to the operation, to as many as five fits in a day. Since the womb has been replaced in its normal position she has become pregnant, and the future course of this case will demonstrate the influence that shortening of the round ligaments will have on pregnancy. She has now reached the fifth month.

Dr. Alexander's paper is full of practical and useful hints as to the best mode of retaining the ligaments after they have been pulled out, and also as to details of subsequent treatment. In concluding, he says, "It is a certainty that up to June last I never heard that any surgeon had proposed to utilise the round ligaments in the correction of uterine displacements. From a paper in the *Glasgow Medical Journal* of that date, by Dr. Jas. A. Adams, Demonstrator of Anatomy in the Glasgow University, I find that he has been working at the same subject, and that he had attempted to perform the operation on the living subject between two and three months after the date of my first operation, but failed, owing to adhesions. . . . . . The matter is not one of any importance, but I freely admit Dr. Adams' claim to originality to be as great as my own, and hail with pleasure his powerful support to what I have no doubt will come to be a useful operation."

Bromide of Ethyl during Labour.—M. Lebert, who has been employing bromide of ethyl largely in midwifery cases, accords it great value in simple confinements. It diminishes and finally suppresses the pain, without having any hurtful effect upon the mother or upon the child. He states also that under its influence labour is more rapid and surgical interference rendered less necessary. The subsequent recovery he believes to be speedier, and the tendency to flooding much less than when the drug is dispensed with.—*Lyon Médical.* 1883. —J. A. A.

Laparo-Colotomy for Removal of a Foreign Body.—At the German Surgical Congress, held in June last, Uhde (of Braunschweig) showed a wooden tool he had removed from the left colon of a man, aged 25, by laparo-colotomy. The tool was one used for sewing sacks, and measured 9¼ in. in length, with a circumference at its thicker end of 3½ inches. The patient, an inmate of a house of correction, had pushed this into his rectum on the 1st June, 1882, and left it there. On the 25th June a sudden movement of the body was followed by abdominal pain, and he found that the tool after that was not so easily felt from the outside of the abdomen as it had previously been. In July he suffered from feverishness, diarrhœa with bloody evacuations, tenesmus, and difficulty of micturition. In October there were rigors, with severe pain in the left hip and thigh. The pain increased so much that on 6th April, 1882, he came to the hospital at Braunschweig for relief. At that time the foreign body could be felt through the abdominal parietes to the inner side of the left antr. supr. spine of the ilium. On the next day an unsuccessful attempt was made to reach it by introducing the hand and part of the forearm into the rectum. On the 13th April an incision was made in the left lima semilunaris, through which the foreign body was felt within the descending colon, jammed between the last rib and the sacrum so firmly that the colon was only brought to the wound by considerable traction. The piece of wood was removed through an incision in the gut nearly 1½ in. in length, and this incision was closed with silk sutures. The operation was performed antiseptically, and, in spite of considerable vomiting and hiccough during and after the operation, the patient made a good recovery.

Similar operations, both successful, were performed by Reali in 1848, and by Studsgaard in 1878. In the former case the foreign body had been in the bowel for nine days, in the latter for twenty-four hours. In the present case it had been there for 317 days.—*Beilage Z. Cbl. f. Chir.* 1882. No. 29.—D. M'P.

THE

GLASGOW MEDICAL JOURNAL.

No. IV. April, 1883.

ORIGINAL ARTICLES.

NOTES OF TWO CASES OF TUMOUR OF THE LARYNX, IN ONE OF WHICH THE TUMOUR WAS SUCCESSFULLY REMOVED BY THYROTOMY.

By D. N. KNOX, M.A., M.B.,
Lecturer on Surgery, Western Medical School, Glasgow.

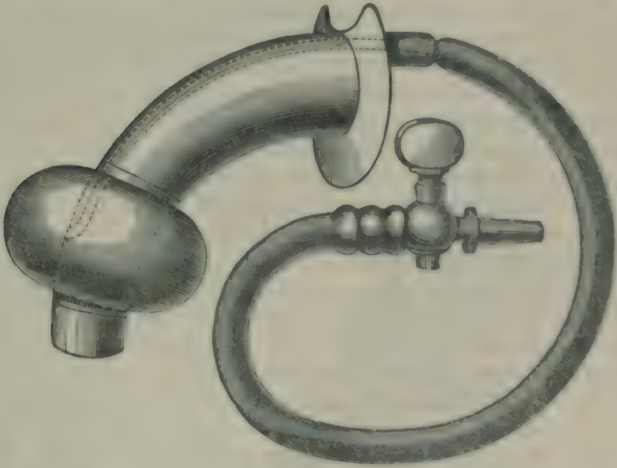
(*Read before the Southern Medical Society, 11th January, 1883.*)

Tumours of the larynx are not so common but that notes of the two following cases may be of interest to the members of this Society. Previous to the discovery of the laryngoscope in 1857, the number of cases reported in medical literature was considerably less than 100, but since that date several hundreds have been recorded as occurring in the practice of a single surgeon. The tumours chiefly found are papilloma and fibroma among the benign, and epithelioma, encephaloid cancers, and spindle-celled sarcoma among the malignant. Of the former class, papilloma is by far the most frequent. Morell Mackenzie states that out of 100 tabulated cases of benign tumour, 67 were judged to be of this character. In appearance, these tumours are generally pink or white; they are usually sessile, and often multiple. Their most common size is that of a large split pea. Their favourite seat is the vocal cords. From this being their position, they are easily seen by the laryngoscope, and their nature easily made out.

As regards symptoms, the presence of a tumour is usually indicated by some alteration of the voice, such as hoarseness or aphonia, paroxysms of dyspnœa, and croupy cough. There may also be other symptoms, such as dysphagia, and the feeling of a foreign body in the larynx, according to the site and size of the tumour. But these functional derangements are very deceptive, while the information given by the laryngoscope is at once precise and sufficient. This fact was illustrated in the first of the cases I have to bring before you to-night. The patient suffered from frequently recurring attacks of loss of voice and laryngeal catarrh for thirteen years. His age now is 41. He was treated for laryngitis by every physician into whose hands he came, till at last he was referred to Dr. Joseph Coats, about a year ago, who at once diagnosed a papilloma growing from the under surface of the left vocal cord. This tumour was considerably larger than the average, being about the size of a horse bean. Dr. Coats succeeded in removing a considerable part of the tumour by forceps inserted through the mouth, and gave the patient so much relief, that he was able to resume his employment as a sailmaker on board ship, and take a voyage to Australia and back. On his return home, however, he felt a renewal of his symptoms, and again came to see Dr. Coats, who, on examining him, saw that the tumour had grown still larger than before, and was pressing upwards the left vocal cord, especially in front. It was sessile, and from its extent, Dr. Coats did not think it could be removed *per vias naturales.* The patient was, however, very anxious to have something done. His voice was completely gone, his occupation gone, and though he had not suffered from paroxysms of dyspnœa, yet from the situation of the tumour, these were very likely to happen, and, besides, Dr. Coats was afraid that the tumour might develop an epitheliomatous tendency. He therefore asked me to see the case with him, with a view to removing the tumour by some extra-laryngeal method. This I did, and agreed to operate.

I may here say that there are two methods of operating in such a case as this. (1.) Laryngotomy or tracheotomy is performed, and after a few days a pair of canula forceps are passed through the opening thus made into the interior of the larynx, and the tumour torn away piecemeal. This method is perhaps the safest and simplest; but it is obviously the most difficult practically, and you cannot be certain that the tumour is eradicated. You are working totally in the dark, grabbing at a tumour in a very confined space, and will probably require to repeat your operation three, four, or even six times, as some of the most experienced operators have done before complete removal is effected. (2.) The other method of operating is more heroic certainly, but you work in the light, and the manual difficulties are reduced to a minimum. I refer to thyrotomy. This consists in slitting up the thyroid cartilage, laying the larynx open, and removing the tumour in the most thorough manner possible. This operation has proved very successful as regards the removal of the tumour, but it has one great disadvantage: in the majority of cases (53 per cent, according to P. Bruns) the voice is lost or much impaired. This was a serious matter in the present case, where the operation was necessitated chiefly by the loss of voice. Guided, however, by the dictum of Bruns in relation to such operations, "that everything depends (so far as restoration of function is concerned), on the question whether the operation can be performed without the division of the thyroid (*i.e.*, the anterior commissure of the vocal cords)," I devised the following operation, which consists of laryngotomy combined with partial thyrotomy, and I have every reason to be satisfied with the result, as it has been, so far as I know, a complete success.

Assisted by Dr. Coats and Dr. Dickson of Paisley, I performed the operation on 17th September last. Tracheotomy was first performed above the isthmus, and then this *tampon*,



made for me by Mr. Hilliard, was introduced into the trachea to prevent blood passing downwards. The tampon is a very simple affair. It is merely a large tracheotomy canula, with a thin piece of india-rubber tied round the lower end about ¼ in. from the margin. This is inflated by blowing through a fine silver tube (portion of No. 1 catheter), which runs along the inner surface of the canula to an opening beneath the rubber. There is no complicated mechanism about it; it is easy of insertion, and is quite efficient. The rubber, being distended, fills up the trachea, and so prevents the passage downwards of blood. The late Dr. Foulis used in his cases a piece of lead tubing, the short arm of which he inserted into the trachea, but this necessitated the having several sizes beside you, so as to get one to fit, or one had to do as Mr. Lister did, in a case reported by him—roll a ribbon of elastic tissue round the end of the tube till the proper size was reached. I did not care to perform these experiments, and so adopted the tube I show you. I may say the only precaution necessary in using the tampon is in blowing up the bladder—not to make it too large, so as to distend the trachea, or reflex spasms are at once set up which interfere greatly with the operation. They subside, however, by slightly reducing the pressure. A notch, which you see in the upper edge of the shield of the tampon, is intended to allow of the insertion of the knife, if necessary, into the tracheotomy wound when about to make the next incision.

There was, as usual after tracheotomy, a good deal of coughing, but not more than in an ordinary case. When this subsided I proceeded to clear the anterior surface of the cricoid and thyroid cartilages, and the crico-thyroid membrane in the middle line. I then cut these structures through from below upwards with a short, strong, slightly curved and probe-pointed knife. I did not cut through the thyroid completely, but left the upper third with the anterior commissure of the cords intact. This, of course, hindered to some extent my view of the interior, but the section of the cricoid amply made up for this. The sides of the larynx were now held apart by retractors and the tumour came partly into view. A small mirror was then placed in the lower part of the wound and thus the tumour became completely visible. I removed it with small curved scissors. It was somewhat larger than a horse bean, sessile, and attached firmly to the left vocal cord and the commissure. After careful examination with the mirror to see that nothing was left, the mucous membrane was cleaned, and the cartilages were allowed to fall together. This they did very perfectly, no deep sutures being required. The integuments were then stitched together in the usual way, the tampon removed, and a large tracheotomy tube substituted, and the patient removed to bed.

His after progress was most favourable. For the first two days he could not swallow anything without great difficulty, as, owing to the inflammatory swelling about the glottis, fluids trickled down into the larynx and set up violent coughing. There was also some slight pain and tightness about the chest, and a good deal of coughing for two or three days, but these subsided under the use of poultices, and without leaving any bad results whatever. The coughing set up some surgical emphysema by forcing the air through the wound in the thyroid under the integument, but this never was of any consequence, and subsided spontaneously in a few days. The tube was removed on the third day, and the wound healed rapidly, except the lower part, which did not close for nearly four weeks, when the patient left for his home in England. Before he left he had almost completely recovered the use of his voice; he still had a little hoarseness, due, as Dr. Coats stated in his report, to inflammatory swelling, but he felt quite comfortable and in as good health as ever he had been A fortnight after he went home he wrote me saying that his voice was still improving, although he had caught a little cold in the journey, which had delayed his progress slightly. Before he left Glasgow, Dr. Coats examined him laryngoscopically, and reported that "the cords were not represented by silvery bands as in the normal condition, being red and obviously still somewhat inflamed, but they moved into the middle line quite in the normal fashion during vocalisation."

With regard to the performance of tracheotomy in such cases, either some time before, or simultaneously with the thyrotomy, I see that Mackenzie and Bruns, the two chief writers on the subject, say that it is not at all required. Curiously enough, however, both of these surgeons speak of the danger to life from hæmorrhage as one of the risks of the operation, and mention several cases where patients nearly died from suffocation with blood during the operation. My own experience in this case showed that tracheotomy was amply justified both at the time of the operation and afterwards. There was not a great deal of hæmorrhage at the time of the operation, but there was enough to have caused a good deal of coughing, and it was necessarily very difficult to stop it till it did so of itself. But it was especially after the operation that the tracheotomy was justified, when, owing to the swelling of the vocal cords, and the parts above them, the difficulty of breathing would have been such that I consider my patient would have been in great danger but for the tube in his trachea.

As regards the question of recurrence there are no large data to found upon. Mackenzie and other writers are agreed that when the tumour is solitary there is much less tendency to return than when it is multiple. Mackenzie gives five similar cases to mine, in which thyrotomy was performed with only *one* recurrence. In 31 solitary tumours, where laryngotomy was performed, there were 24 cures and 7 recurrences. On the other hand, when the tumours are multiple, there are nearly as many recurrences as cures under either operation.

Lastly, unless further experience of the operation I have now described should bring presently hidden dangers to light, I think I may claim in the meantime for it the following advantages.

1. It leaves the vocal cords completely undisturbed.
2. It renders sutures through the sides of the thyroid cartilage to keep them in position unnecessary, and thus diminishes the risk of perichondritis.
3. It gives easy access to the interior of the larynx, and allows therefore of the rapid and complete removal of new growths at one operation.
4. As a corollary from the above, it is especially valuable in cases of multiple growths, which may by this method be removed almost as easily as solitary ones. For I do not think that thyrotomy has as yet received fair play in the matter of statistics, having been hitherto reserved for the very worst cases. If it were more frequently performed, and at an earlier period, I believe its results would be better than those of either operation as hitherto performed.

Epithelioma of Larynx.—The next specimen I have to show you is an example of the most common kind of malignant growth in the larynx—viz., *Epithelioma.* The patient was under the care of Dr. J. A. Dickson, of Paisley, by whose kindness I was enabled to see him before death, and from whom also I received the specimen. The history of the case, as given me by Dr. Dickson, is shortly as follows.

John B., æt. 26, miner, was admitted to the Glasgow Royal Infirmary on 2nd December, 1881. He stated that, four years before, he caught cold working in a damp mine. The cold went away, leaving a stiffness in the chest which gradually increased up to time of admission. His voice also became affected, and he had a short dry cough, worse at night. He had considerable pain on speaking, coughing, or swallowing solid food. This pain was present more or less throughout, and radiated upwards and outwards to the parotid region. On the night of the 21st January, 1882, he became suddenly dyspnœic and seemed about to suffocate. Tracheotomy was therefore performed to the entire relief of the symptoms.

The tube, however, could never be afterwards dispensed with.

The larynx was carefully examined, and it is stated that "there was slight œdema of epiglottis and vocal cords so as almost to close the windpipe." No tumour was then discovered. There was nothing to be felt externally. Iron and potash, iron and mercury, &c., were given him internally, and nitrate of silver, blisters, and scarification, were used locally for some months, and in April the patient was sent to the Convalescent Home prior to having thyrotomy performed. When he returned, however, he was still too weak to undergo any operation, and so was again sent to the country. At a later period he became a patient in the Paisley Infirmary, and there he died.

The hoarseness, dysphagia, pain, and occasional attacks of dyspnœa were marked symptoms throughout, and the œdema continued about the same during the whole of his illness. About a week before his death a small, warty, papillomatous-looking growth was noticed projecting through the rima glottidis. This was the first appearance of a new growth. During the last month all the symptoms became much worse, fluids only could be swallowed, and of these a small quantity passed into the trachea, causing acute dyspnœa and coughing. Everything was done by way of enemata, &c., to sustain the strength; but vomiting came on, and the patient rapidly sank, and died on 25th September, 1882.

Post-mortem examination of the specimen shows that the entire larynx and upper part of trachea are nearly filled with a new growth, which the microscope shows to be epithelioma. The true vocal cords have been destroyed, and the false vocal cords, epiglottis, and ary-epiglottic folds are thickened and infiltrated with the new growth. Thus, the so-called œdema is accounted for. The posterior part of the cricoid has been also destroyed by the ulcerative process, and an opening formed into the œsophagus, while the rest of the mucous membrane is covered with warty vegetation.

I have very little further to say. The disease seems to have begun about the cords or ventricles of Morgagni, and seems to date back to that cold caught in the pit. As a rule, we know very little about the origin of cancer when it appears in any organ, but you know how frequently patients attribute tumours, say of the mamma, to blows and other slight causes. It is possible here that a severe catarrh, together with the debilitating influences of a damp mine, may have been at least the predisposing, if not the actual exciting causes of this disease. I may add that in most of the recorded cases the early symptoms are usually those of a simple catarrh, and this leads to great difficulty in the diagnosis. I think I am not wrong in saying that this difficulty was experienced in the present case, and that for a long time the patient was supposed to be suffering from chronic laryngitis with simple ulceration. This opinion would no doubt be supported by the patient's age, which is only 26, a very early age for epithelial cancer to make its appearance. Looking back now over the whole history of the case, the story of catarrh followed by gradually increasing hoarseness, dyspnœa, dysphagia, pain radiating upwards and outwards from the larynx, with fœtor of the breath, and cachexia, presents an almost perfect picture of malignant disease of the organ.

ON VENESECTION.

By JOHN WYLIE, M.B., Stewarton.

DURING the past twelve months I have read with much pleasure several interesting articles in the various medical journals in favour of blood-letting, and as I have paid careful attention to the subject for the last five or six years, I have thought it might not be considered presumptuous in me to record a few cases, in which I have found very great advantage accrue from its adoption.

In an old *Encyclopedia of Practical Medicine*, published in the year 1833, I find an interesting and instructive article by *Marshall Hall*, which he introduces by saying that general blood-letting is of all our remedies the most powerful, but that its employment requires the utmost consideration and precaution. The principal circumstances, he says, which require to be attended to, are:—

1. The nature and stage of the disease.
2. The velocity and force of the heart's action.
3. The condition of the pulse at the wrist.
4. The appearance of the blood.
5. The effect upon the patient in subduing the disease or inducing syncope.

Now, I venture to think that if we, in this age of progress, with fifty years' more experience, would only carefully and without prejudice consider these circumstances, and use due discrimination in the selection of our cases, we would be compelled to admit that blood-letting is still one of our most powerful remedies. Within the last few years there has been a gradual reaction in its favour; and now, with our more advanced knowledge of physiology, the indications for resorting to it are much more precisely defined.

I shall never forget my first experience of blood-letting; the circumstances are so indelibly fixed in my memory, that although six years have elapsed I could almost relate them without referring to my notes. M. M., a strong healthy young man, the son of a farmer, had been suffering for three days from a severe attack of capillary bronchitis. I had prescribed the usual remedies—expectorants and emetics, with poultices and turpentine stupes externally, but the symptoms continued to grow more unfavourable, and at last assumed such an alarming aspect that the patient seemed not likely to live many hours. His breathing was 65 per minute, pulse 130, small and feeble. His face had a peculiar blueish, bloated, anxious appearance, and his body and extremities were mottled with blackish smoky coloured spots, and covered at the same time with a cold clammy perspiration.

His father, an old man leaning on his staff, stood by the bed-side, and with tears trickling down his cheeks implored me to bleed him. Although I had then been a graduate for four years, and had spent two years in an English hospital, and two years as assistant in a large city practice, I had to confess that I had never seen the operation performed. However, I thought it might relieve the pressure on the right side of the heart, and give at least a little temporary relief, so with the old man's assistance I bared the arm and opened the vein. The blood at first came with great difficulty, being thick and black, almost like tar; but in a short time it flowed more freely and became less livid. As the blood flowed the patient seemed to rally, his appearance became more natural, his breathing much calmer, his pulse steadier, and when about fifteen ounces had been withdrawn, he expressed himself as feeling almost well. Twenty minutes afterwards his breathing was thirty-four per minute, pulse ninety-eight, and the anxious oppressed look had given place to a pleasant smile. The relief continued for twenty-four hours, but at the end of that time he again became oppressed, and when thirty-six hours had passed the old symptoms had returned to such an extent that I resolved to bleed him again without further delay. By the time twelve ounces had been withdrawn, he declared that he found himself much better. He continued thenceforth steadily to improve, the bad symptoms were never manifested again, and within a week he was able to move freely about his room.

This was the means of turning my attention to the subject, and I longed and watched for other cases. During the last five years I have bled by the arm over fifty persons, besides practising local depletion by leeches and cupping. These have been almost exclusively farmers and members of their families, or persons employed in agricultural labour. The cases in which I have found most advantage to be derived from its employment are acute cases of pleurisy and pneumonia, peritonitis, hepatitis, and nephritis.

One of my patients was at one time suffering from aneurism of the aorta, and when acute pain and dyspnœa came on—caused by too much exertion, I bled him three times, affording great relief to him by the operation.

On several other occasions, when the right side of the heart was labouring and requiring relief, I resorted to blood-letting, and the same beneficial results invariably ensued. I will very briefly give my notes of a few cases.

W. D., a stout florid-complexioned farmer, had been indulging rather freely in the new year's festivities, and when going home had sat down by the roadside and fallen asleep. When a friend awoke him he was shivering all over, and afterwards complained of a severe darting pain in his left breast. When I saw him two hours after his skin was hot and dry, pulse 96, hard and full, respiration short and hurried; he had also a short harsh cough, which tormented him terribly, and made him cry with pain. Auscultation detected the usual friction sound of pleurisy, at a point just below the nipple, over the sixth intercostal space.

I at once bled him to the amount of sixteen ounces. The pain almost immediately left him, and he stated that he felt himself much better. I then gave him ten grains of Dover's powder, along with ten grains of salicylate of soda, and applied at the same time a warm poultice over the side. Next morning I found him progressing favourably. During the night he had perspired freely, and had felt no pain unless when taking a deep inspiration. In three days he was able to move about the house.

T. S., a ploughman, fell from a cart, and lighted on his shoulders and head. He was stunned, but soon rallied and walked home. Next day he felt stiff and sore about the shoulders, and the following morning he had a rigor. When I saw him about noon his skin was hot and dry, pulse 110, and he was complaining of a severe pain in the head. His face was alternately flushed and pallid, conjunctivæ injected; there was great restlessness, and already, when left alone, his mind wandered, and he talked incoherently. He had also vomited several times. As the case seemed to me one of acute meningitis, I resolved to bleed him at once, and drew from his arm about fifteen ounces of blood. He seemed relieved, and lay perfectly quiet for about half-an-hour. He then appeared to rally, and to regain consciousness, and expressed himself as feeling very much better.

I applied ice to the head, mustard to the nape of the neck, and administered to him a diaphoretic powder. From this time he continued to improve and to make a good recovery.

N. W., a stout domestic servant, got a severe wetting while menstruating, and neglected to change her clothing. She had a rigor about midnight, and afterwards complained of acute pain in the hypogastric region. This soon spread over the whole abdomen, and was increased by pressure, and attended by high fever.

When I saw her in the morning, the temperature was 103°, pulse 120, small and weak, respiration hurried, and her countenance anxious and expressive of suffering. She lay on her back, with her legs drawn up, and could scarcely bear to be touched. The abdomen felt tense and tympanitic. I at once bled her from the arm, to the amount of twelve ounces. She said she felt very much better, and at the same time her countenance changed and her breathing became quite natural. Half-an-hour afterwards she was lying perfectly quiet, and almost free from pain. I treated her in the ordinary way with opiates, sedative fomentations, ice, and light diet. From that time she continued to improve, and to make a rapid recovery from what undoubtedly threatened to be a serious attack of peritonitis.

With due discrimination in selecting cases, venesection is certainly a safe remedy. I have never seen it do harm, and on every occasion on which I have resorted to it, the patients have spoken in no measured terms of its beneficial results on them. I may mention that it is a remedy in which farmers especially have the greatest faith. I know at least of two cases where the farmers themselves bled each other unknown to the medical attendant, who had refused to do it.

I can well believe that medical men in large cities have fewer opportunities than we in the country have for the practice of venesection. In agricultural districts, where we meet with acute inflammation, in full-blooded over-fed farmers, and their servants, I believe, there is no other remedy that has such a powerful influence in temporarily arresting the progress of the disease, and allowing us an opportunity of applying the ordinary means to keep the disease under control.

TISSUE REPAIR, OR THE PATHOLOGY OF THE SUBCUTANEOUS OPERATION BY INJECTION FOR THE CURE OF HERNIA.

By JOSEPH H. WARREN, A.M., M.D.,
Member of the British Medical Association; Member of the American Medical Association (Judiciary Council); Hon. Member of Vermont State Medical Society; Member of Massachusetts Medical Society.

Part I. On Inflammation and its Relation to Tissue Repair.

"The subject of irritation being the foundation of surgical science, you must carefully study and clearly understand it before you can expect to know the principles of your profession or be qualified to practise it creditably to yourselves, or with advantage to those who may place themselves under your care."—Sir Astley Cooper, *Lectures on Surgery.*

Theories of Inflammation.—"No topic in medicine has been the subject of so much research and speculation as the nature of inflammation; but even at the present time it is impossible to give a complete and correct definition of inflammation from a pathological or an etiological standpoint." (Flint.) The word originated when there were no true pathological ideas; yet even the oldest observers regarded the process as an intense increase of the normal physiological processes—a "disturbance of nutrition." (Virchow.) The idea, however, that the classic symptoms, *tumor*, *rubor*, *calor*, *dolor*, pain, heat, redness, and swelling, are the true characteristics of inflammation, has unfortunately become so deeply rooted, that it may seem to some almost absurd to claim that the inflammatory processes may be present in tissues even when the local *gross* symptoms are absent. The modern histological idea of inflammation as a purely *regenerative* process, which directly or indirectly replaces the loss of substance, cannot. indeed, be rigidly separated from the classical idea of an intense irritation which tends directly or indirectly to a *destructive* process. The difference is only in the *degree* of irritation; but this is a difference so fundamental and important for the proper understanding of the processes of tissue repair, that it must be made emphatic. The word inflammation is, however, in such common use, that it would be foolish to attempt to discard it. I shall, therefore, for the clearness of discussion, designate the *salutary* process REGENERATIVE INFLAMMATION, and reserve for the *intense* process the term CLASSIC INFLAMMATION.

According to John Hunter, and to the modern French surgeons, the essential phenomena are referable to the vascular system. Plastic lymph—that is, a thin opalescent layer containing fibrin and red and white blood corpuscles, is derived from the vessels by exudation, and is susceptible of organisation, and of the formation of the different tissues of the body. Recently this theory has been revived, in a modified form, by Cohnheim, who explains the process by an exudation of serum and white corpuscles, and a coagulation of a fibrinogenous substance contained in the exuded serum.

Plastic Lymph Theory.—The former ideas of this effused lymph divided it into two distinct classes, which have received various names:—

Healthy (Clarke).	**Unhealthy.**
Plastic (Williams).	**Aplastic.**
Fibrinous (Paget).	**Corpuscular.**
Fibrinous (Rokitansky).	Croupous.
Coagulable.	**Degenerative.**
Adhesive.	**Suppurative.**

According to this theory, which held undisputed sway for many years, and which is even at the present day often confounded with later and more exact theories, there are two elements in the exudation—namely, fibrin and corpuscles, which control the development of this lymph, so called. In proportion as fibrin is abundant, so will the tendency to organisation and adhesive inflammation prevail; but in proportion as the blood corpuscles are abundant, so will the tendency to degeneration and suppurative inflammation prevail. Ever since the time of Bichat there has been a general impression that each tissue has its proper mode and product of inflammation, *serous membranes being prone to fibrinous exudations, mucous to suppurative or corpuscular, and cellular to both fibrinous and corpuscular.* Slight inflammations also were believed to be almost always corpuscular, while the more acute and active were fibrinous.

Repair by means of *nucleated cells* was said to be observed in granulations, in inflammatory adhesions (either in *serous sacs* or in healing by first intention), in inflammatory indurations, and in certain naturally developed connective tissues. The process was accurately described by Schwann, who saw the nuclei become more distinct, and then have several nucleoli appear in them. The cells, moreover, became granular and elongated, and grouped together in clusters or filaments.

Development of *fibrous* tissue was supposed to take place by means of a *blastema*, and was best seen in subcutaneous wounds.* It was the only mode of formation of connective tissue recognised by Henle.† A similar process is described by Reichert, Zwicky, and Gerlach. The first thing noticed after the exudation was a quantity of molecular or finely shaded substance like homogeneous or dotted fibrin. At first there were no nuclei or cells apparent, but later they began to appear as oval bodies, soon becoming elongated. They had clear contents and no nucleoli, and were very firmly embedded in the blastema. These nuclei underwent very little change. except to elongate and possibly to become irregular in outline. but the blastema assumed a more and more filamentous appearance, and finally a filamentous structure. Henle, indeed, supposed the nuclei to be made into fibres, but of this there does not seem to be proof. The exudation of cells which, of course, to a limited extent accompanied this blastema, was considered to have no share in the healing process. In fact, the more abundant the cell exudation, the longer the inflammation; and the longer the inflammation, the less speedy and perfect was the repair. The theory of a blastema is now, however, antiquated, and believed to be histologically incorrect.

Proliferation Theory.—Virchow, on the contrary, rejecting Hunter's doctrine, maintained that the chief effect of an inflammation is an increased function and nutritive activity of cells in the solid tissues outside the blood-vessels, and that hyperæmia and exudations from the blood are secondary changes. This theory rests on a *proliferation* from pre-existing fixed cells, in accordance with the law "omnis cellula e cellula," and is to-day supported by Stricker, Burdon Sanderson, and their school.

Cohnheim's Theory.—The discovery by Recklinghausen, in 1863, of wandering cells in the tissues, rendered Virchow's theory ‡ somewhat doubtful, while a new era was introduced in the history of inflammation by Cohnheim's discovery § of the emigration of white blood corpuscles. This migration of cells had been first observed by Dr. W. Addison, ‖ and again by Dr. A. Waller; ¶ but their isolated observations were little thought of and soon forgotten. This is not the place to detail the theory of Cohnheim, which is already accepted as scientifically accurate, nor to discuss the exact method of the emigration of these cells. The former is too well known to need description, the latter is still a question of some dispute.

---

* Paget, *Surgical Pathology.*

† *Allgemeine Anatomie.*

‡ "Uber Eiter und Bindegewebskörperchen."—*Virch. Archiv*, xxviii.

§ "Uber Entzündung und Eiterung."—*Virch. Archiv*, xl, 1866.

‖ "Experimental and Practical Researches on Inflammation."—*Trans. Prov. Med. Assoc.*, 1842.

¶ *Phil. Magazine.* Vol. xxix, 1846.

That the greater number of these cells is undoubtedly the result of an emigration of these white blood corpuscles is, however, by no means incompatible with the theory that the fixed cells of connective tissue become amœboid, proliferate, and produce other cells, that is, take part in the regenerative process.

Organisation.—The nature of this cell activity is conservative. Whenever a living tissue is irritated, provided the irritation be insufficient to destroy the vitality of the part, an inflammatory process is set up which tends to recovery, and is indeed the one process indispensable and necessary for recovery. These cells, which have been produced by a process of emigration, or of proliferation, or by both processes combined, do not, however, remain stationary and inactive. They tend to become either *organised* into higher structures, or *disorganised* into a state of fatty degeneration and *absorption*, or else a state of *suppuration*. The tendency to undergo a progressive organisation into higher tissues will vary:—

1. With the tissue involved.
2. The condition of the individual.
3. The degree of inflammation.

In non-vascular tissues, such as cartilage, tendon, and the cornea, exudation from the neighbouring vessels is limited, and hence the effusion is small in amount, while in vascular areas, as bone and the loose connective tissues, the inflammatory effusion is more abundant. "The more intense, however, the inflammation, the more abortive are the young cells, and the less their tendency to form a permanent tissue." (Green.)

Resolution.—On the other hand, if the injury to the tissues be very slight, the inflammation may gradually subside, the emigration cease, the young cells become fatty, degenerated, and little by little pass into the lymphatic circulation, and leave the tissue in its normal condition.

Suppuration.—If, however, the irritation be so severe or prolonged as to prevent either resolution or organisation, or if the condition of the individual be unfit to produce a favourable termination of tissue lesions, the cells will accumulate in such numbers, and so degenerate as to become purulent in their nature and behaviour. In regard to the influence of constitutional peculiarities over the tissue changes in inflammation, clinical evidence is not wanting. The taint of various diatheses, the scrofulous so called, the syphilitic, cancerous and tuberculous are well known to impart an abnormal susceptibility of certain tissues to injury or irritation, and a peculiarity in the products and course of the inflammation which such irritation produces. There is little or no tendency to the organisation of healthy new growth. From facts such as these, the conclusions have been drawn that in young persons, and in persons of strong constitution, we may, as a rule, look for a good and permanent repair of tissue lesions, a so-called "*adhesive inflammation*," but that in the old and cachectic we may expect a *suppuration*.

The deleterious influence which pus exercises upon surrounding tissues and the process of repair is also a well known clinical fact. Pus, which has remained for any length of time in the tissues, undergoes certain changes. It may undergo fatty metamorphosis, and thus, by absorption, poison the system. At any rate, " the pus corpuscles appear to be endowed with the power of absorbing the tissues with which they come in contact, or at all events of causing their liquefaction." Hence, the softening and disintegration of the tissues which constitute such a destructive element in intense inflammation.

Subcutaneous Healing.—The problem of modern surgery has been to create a sufficient amount of inflammation to produce the amount of exudation requisite to ensure tissue organisation, but not to exceed this sufficient amount of exudation, and so produce an *intense* inflammation with all its purulent sequelæ. This problem has been solved by the introduction by Strohmeyer and Dieffenbach of the method of subcutaneous operations. In fact, the fundamental and only essential principle of Listerism is the exclusion of outside foreign material, that is, the transformation of an open wound into a wound practically subcutaneous.

The advantage of a subcutaneous over an open wound is this, the former heals by *primary adhesion*—the "First Intention" of Hunter—other conditions of the patient being favourable; while the latter, under the same conditions, might heal by primary, but would probably heal by *secondary adhesion*—the "Second Intention" of Hunter—that is, by granulation and suppuration.

What, then, are the processes we observe in union by primary adhesion after a subcutaneous operation? As a result of the operation, the conditions of the vascular circulation have been altered. The liquor sanguinis which has thus escaped coagulates within twenty-four hours, on account of the fibrin elements it contains, and imprisons numbers of white corpuscles. The stiffening of this intercellular substance explains why the union is so firm as to unite without sutures. The cells in the wound gradually become spindle-shaped, and assume more and more the form of normal fibrous connective tissue, becoming stellate and anastomosing into a firm network.* However rapid the union may be, it is not so simple as some authors have thought.†

Formerly, under the old plasma theory, as I have previously said, it was supposed that the fibrin which was exuded in the liquor sanguinis was changed directly into fibrous tissue. Schwann assumed that there existed originally spindle cells from which bundles of connective tissue were developed by a splitting of the body of the cell. Henle thought that originally there were no cells, but only nuclei, and that the fibres which afterward appeared were produced by direct fibrillation of the blastema, while the nuclei gradually became elongated, and coalesced to form longitudinal fibres (*Kernfasern*—nucleus fibres). Reichert contended that there were both cells and intercellular substance, but said, "the intercellular substance fibres were a false interpretation of an optical image." Virchow, with Schwann, and against Henle, believes that spindle-shaped cells indisputably exist, and with Henle and Reichert, and against Schwann, that a splitting up of the cells into fibres does not occur, but that a previously homogeneous or mucous intercellular substance becomes fibrillated in some way, while the young cells maintain their integrity throughout. It is unnecessary to say that this is a manifest inconsistency in Virchow's *Cellular Pathology*.

Max Schultze of Germany, Beale of England, and Leidy and Tyson of America, believe that the intercellular substance originates by a change in the nucleus at its periphery, and by a pushing off of this formed material from the cell. This theory seems to be the most consistent with observation, and is the one that is generally adopted at the present day.‡

The intercellular substance having thus been formed has yet to undergo other processes before it reaches a stable state. The spindle cells assume the flat shape of connective tissue corpuscles, and coalescing into bundles or fasciculi, with the nuclei alone remaining, gradually render the newly formed tissue firmer and denser by the contractile power of fibrous tissue, until at last it is as solid and compact as normal tissue.

---

* Kolliker describes the white fibrous tissue as formed from the coalescence of fusiform cells, and the yellow elastic fibres as developed from the stellate branching corpuscles, which Virchow has called connective-tissue corpuscles.

† Cornil & Ranvier, *Path. Histology*, p. 71.

‡ Tyson, *The Cell Doctrine*, p. 141.

and remains stable in this state. That this contraction and stability of the new fibrous tissue is not merely theoretical but is emphatically proved by clinical experience, will be later clearly shown.

Healing by Granulation or Suppuration.—All wounds do not, however, heal by primary adhesion, and especially is this statement true of wounds where open incisions are made through the cutaneous tissues to the deeper parts. The process of repair in such wounds is rarely other than by *secondary adhesion* or by *suppuration.* Histologically the process is the same in character as in repair by primary adhesion, but it differs greatly in degree; the cicatrisation also is slower, and the tissue may experience the divers accidents incident to suppurating wounds. When, however, inflammation is artificially produced in order to gain a regenerative new formation, this difference becomes so important as to throw the two processes of healing into two distinct classes; one of which, the primary adhesion, attains the end for which it is seeking, the other, the secondary adhesion, tends to defeat it. Whereas in the subcutaneous operation we had no external opening; in this case we have an open gaping wound. The flaps are reddened, swollen, and sensitive to the touch, while over their whole extent is seen a number of yellowish or grayish particles. These particles are dead bits of tissue still adherent to the living portion. Particularly is this the case, says Billroth,* if tendons, or fasciæ, or the cortical substances of bones have been injured, and if the wound is upon old or debilitated persons or badly nourished children. Under such circumstances as these, the process of repair will be much prolonged. Until, then, these irritating bits of dead tissue "clean off," no active process of repair can begin. Supposing repair to have begun, we shall find the wound gradually filling up from the bottom, with a mass of cells or granulation tissue. These granulations are covered all the time, however, with a fluid which becomes thick, pure yellow, and of creamy consistence.

This is pus, and in this lies the danger and the weakness of the granulation process. In most cases, more or less of this pus will die from insufficient blood supply. The cells escaping from the vessels remain upon the surface of the tender granulations, bathing them in a soft gelatinous fluid. This fluid tends to disintegration and re-absorption into the body through numerous lymph channels, and at the same time reacts upon the newly formed granulations beneath it. Some of the cells gradually become fatty and granular, and are absorbed, while others undergo the change into connective tissue already described. At best, the process is a long and tedious one compared with subcutaneous processes, and the danger is always an immediate one, that in place of organisation a suppuration may destroy what little has been built up.

---

* *General Surgical Pathology and Therapeutics.* Amer. Ed. Sec. vii, p. 7.

Laws Governing Repair.—From all that has thus far been said, *six* important laws controlling the transformation of inflamed tissue may be legitimately deduced.

1. A certain amount of inflammation is necessary for repair.

2. Inflammation and repair, although bearing the relation of cause and effect, are antagonistic in their processes; in other words, repair does not, and cannot begin until inflammation subsides, and the longer and the more severe the inflammation, the more weakened will be the tissues, and the less able will they be to organise into higher development.

It is true John Hunter claimed that "injuries in which the parts do not communicate externally seldom inflame, while those which have an external communication commonly both inflame and suppurate." Nor is it inconsistent in the least with what I have just laid down as a general axiom; on the contrary, it rather corroborates it. Viewed with the naked eye, the gross appearances of a subcutaneous wound are those of a total absence of inflammation, but the process that is going on beneath the surface, and at the seat of irritation, is essentially and histologically inflammatory. If, however, this inflammation becomes more intense than nature requires for the healing process, it passes beyond a mere physiological activity of cells, and reveals itself by the classical signs of inflammation—*rubor et tumor, cum calore et dolore.*

3. "The processes of repair are identical in all tissues, such modifications alone showing themselves as necessarily appertain to the anatomy of the tissue or the special circumstance of its position." *

4. The essential element of repair is the growth of embryonic tissue.

5. "Whenever artificial or pathological irritation has determined a growth of embryonic elements, if the irritation cease, this new growth always tends to return to the original form of the tissue, which served as a matrix." †

6. In other words, "whatever may be the origin of this embryonic tissue, it has a tendency to reproduce the tissue of the region where it is seated." ‡

---

* Bryant, *System of Surgery.* † Cornil and Ranvier, p. 72. ‡ *Ibid.*

(*To be concluded in next issue.*)

A CASE OF DOUBLE UTERUS IN THE LIVING SUBJECT.

By FRANCIS HENDERSON, M.D., Glasgow.

MRS. W., who is the subject of this abnormal formation, was under my observation for seventeen years, so that I am in a position to write the history of her case largely from personal knowledge, and my acquaintance with the character of the patient enables me to certify the accuracy of the statements supplied by herself.

Mrs. W., æt. 39, began to menstruate at sixteen, and continued to menstruate at perfectly regular (although abnormal) intervals until her marriage. The menses made their appearance every three weeks, and lasted one week; so that she was always one week *unwell* and two weeks *well*. From the date of her marriage (July, 1865), until six weeks before the birth of her first child in July 1866, menstruation took place regularly every three weeks. The discharge continued about a week and had the ordinary appearance. Mrs. W. considered it *rather more abundant than usual.* This labour, as well as the five subsequent labours, at all of which I attended, was perfectly natural, and required no interference. Mrs. W. nursed her first child for eighteen months, during which period she did not menstruate. On ceasing to nurse, the menses reappeared. She has nursed all her children and has never menstruated while nursing.

In November, 1868, the second child was born. While carrying this child Mrs. W. had a sanguineous discharge nearly all the time, which increased, indeed became profuse, if she did any work. In consequence, she had to lie a good part of her pregnancy, and when labour came on was in a state of great exhaustion from loss of blood. After the birth she soon recovered. The infant presented the appearance of a child at the full term, but as with her first child, she had no means of calculating the duration of pregnancy. In March, 1871, the third child was born. During the early part of pregnancy there was a menstrual discharge at her usual intervals on three occasions. In April, 1873, the fourth child was born. During the whole term of utero-gestation neither menstruation nor discharge of any kind occurred.

Her fifth and sixth pregnancies in 1875 and 1877 respectively, were also perfectly normal.

After weaning her sixth child, Mrs. W. was regularly unwell every three weeks until the end of June 1879, then she passed two periods and considered herself pregnant. About the end of August some bloody discharge made its appearance, and she was "never quite dry" from this time until the end of December.

In October she called on me. She believed herself to be at the fourth month, and her appearance corresponded, but I did not examine her minutely. I saw her next in November. She seemed to me to have fallen in size, and she thought so herself. The bloody discharge still continued, although not great in quantity. My impression was that miscarriage would probably occur, but from the history of the patient's early pregnancies, the constant discharge of blood was of less importance as an indication. By the middle of December the discharge had increased in quantity and the diminution in size was decided, so I resolved to "empty the uterus." There had been no uterine pains, and the *os uteri* was quite firmly closed. Artificial dilatation was accordingly begun by tangle tents and afterwards by sponge tents. At first I experienced difficulty in introducing the tangle tents as they came against an obstruction in the cervix, but I learned by bending them to one side to avoid this. Two days were occupied in getting the *os* and *cervix* dilated so as to admit the finger. I then discovered that fully one inch from the *os externum*, the finger came against a sharp edge which proved to be a septum dividing the upper part of the cervix. Turning the finger to the left side of the patient's body, it entered a small cavity of about one inch deep, the interior of which felt as if covered with a smooth lining. Withdrawing the finger and turning it to the other side of the septum it entered a comparatively large cavity, the surface of which was less smooth. It now became evident that the uterus was double, and that it was the right half that was enlarged, and as I supposed, contained remains of the fœtus and placenta. After getting the assistance of Dr. M'Ewan, of Helensburgh, the patient was put under chloroform, the anterior lip of the os was seized with volsella and drawn down nearly to the ostium vaginæ, which could be done with very little force. The index and middle fingers were then passed into the right compartment of the uterus. Its cavity was easily explored, as it did not exceed 3 or 3½ inches in length, and to our great surprise it was completely empty. All that I could succeed in removing, by gently scraping its walls, was a very small quantity of whitish shreddy matter. We took advantage of the patient being under chloroform to examine very thoroughly the body of the uterus through the upper part of the vagina. A cleft was quite easily recognised, beginning about an inch behind the junction of the cervix with the body, and extending as far as the examining finger could be pushed upwards. The right half being very much the larger.

I shall not attempt to discuss here what was the nature of Mrs. W.'s case during the last half of 1879—whether it was extra-uterine pregnancy or some other form of tumour. The enlarged and empty cavity of the uterus is a strong point in favour of the former. It should be stated that neither prior to this operative procedure nor subsequently was there any solid substance known to have been expelled except blood clots, and no pus escaped *per vaginam.* The discharges were carefully examined. After, and probably in consequence of, the procedure above described, the uterus rapidly contracted and the hæmorrhage was completely arrested. The patient lay in bed for three or four weeks, and one could observe, by vaginal examination, the gradual diminution of the right half of the uterus in size and in sensitiveness to the touch. Prior to the operation the patient had complained a good deal when pressure was made on this part. Before she was able to leave her bed, what proved to be menstruation took place, and from that time it recurred regularly every three weeks until her next pregnancy, two years afterwards.

In the spring of 1880, the late Dr. J. G. Wilson (consulting physician to the Glasgow Lying-in Hospital), being in Helensburgh, I related Mrs. W.'s case, and proposed that we should visit the patient, premising, however, that as fully three months had elapsed since I had seen her, it was possible that from the involution of the uterus, its double condition might not be recognisable. We found Mrs. W. going about her domestic duties in good health. On examination, Dr. Wilson was much gratified by finding that the abnormality was quite distinct. On passing the finger behind the cervix a shallow depression was reached, from which a groove extended through the centre of the body of the uterus. This groove was felt to grow deeper as the finger was advanced towards the fundus. The right half was still somewhat larger and more prominent than the left. As Mrs. W. suffers from a certain amount of falling down of the womb, this examination was easily made. Dr. Wilson found the os firmly closed, and could not pass his finger into the cervix.

Mrs. W. continued to menstruate regularly until the end of 1881, when she again became pregnant. This pregnancy was perfectly normal; there was no appearance at any time of the menstrual discharge. Between the seventh and eighth month I had an opportunity of making a careful examination, but could discover nothing unusual; the cleft in the body of the uterus, recognisable in the unimpregnated state, could not be made out. I had looked forward with interest to the occurrence of pregnancy again, in the hope that I might be able to observe in which half of the uterus utero-gestation took place. During the earlier months it would probably be easy to settle the point, but I was absent from Helensburgh during this period.

Mrs. W. was attended at her confinement, which occurred on the 2nd October, by Dr. M'Ewan. He was twice summoned to see her during the preceding week, and on both occasions failed to discover any signs of double uterus, or indeed of any peculiarity. When labour fairly set in, the child's head was on the floor of the pelvis when Dr. M'Ewan arrived at the house; but a careful exploration after delivery for any of the remarkable conditions which he had observed when assisting me in December 1879 was entirely negative. The child was born alive and is doing well. Mrs. W. went on favourably until the 14th October, when she was suddenly seized with severe pain in the calf of the left leg, which proved to be due to an obstruction in the veins. The whole leg from above the knee downwards became dropsical. The symptoms gradually abated, and the patient was fairly convalescent by the beginning of November. About this time, *i.e.*, fully one month after the birth, Dr. M'Ewan made a careful vaginal examination: he writes me, "about an inch above the junction of the cervix with the body of the uterus I could feel a distinct groove on the posterior surface. I do not doubt that this groove indicates the point at which bifurcation begins." "The *left* segment of the body of the womb is much more developed than the *right*."

Remarks.—There are several points of peculiarity and interest in this case, but I would ask attention chiefly to these two:—I. The existence of a double uterus; and II. The occurrence of menstruation during pregnancy.

I. The observations above described leave no manner of doubt as to the presence of a double uterus in this patient. The division of the cervix by a septum, about one inch above the *os externum*, the ability to pass the finger into two compartments, observed by Dr. M'Ewan and myself, and the cleft condition of the body recognisable *per vaginam*, regarding which the late Dr. J. G. Wilson was also perfectly satisfied; these facts can only admit of one conclusion. But further, it will be noted that, in December 1879, it was the *right* compartment of the uterus that was enlarged; whereas, more than a month after Mrs. W.'s last confinement, in October 1882, Dr M'Ewan finds the *left* segment of the body of the womb much more developed than the *right*. This observation permits the nearly certain inference that, on the last occasion, utero-gestation took place in the *left* compartment. We have no evidence that the *right* compartment ever carried a child, but, from what we know of its capacity, there is good reason for believing that it might do so. Thus, although the *proof* is not complete, there is the highest possibility that *both* compartments are capable of discharging *perfectly* the functions of a normal uterus. If this is so, then not only *may* gestation take place in either or both compartments, but menstruation also may be expected to occur in connection with both divisions.

II. The occurrence of menstruation during pregnancy.

During Mrs. W.'s first pregnancy, this phenomenon was displayed *in the most typical manner*, at the intervals proper to her, until six weeks before the birth of her child. In her third pregnancy, menstruation occurred three times in the early months at her usual periods.

The concurrence of a double uterus and menstruation during pregnancy suggests, that the former is in this case, the explanation of the latter, and that the same explanation may apply to other cases of menstruation during pregnancy. It is obvious that, when a uterus is double, it is not impossible that regular menstruation might take place in one compartment, while gestation was running its natural course in the other.

In estimating the value of this explanation of menstruation during pregnancy, in its relation to observed facts, two questions present themselves for consideration.

First.—What is the relation as to frequency of these two conditions? Are there more cases of menstruation during pregnancy than there are of double uterus?

(*a.*) With what frequency does menstruation during pregnancy occur?

Before this question can be answered, it is necessary to define what is to be understood by menstruation during pregnancy, as this has been a much disputed phenomenon. If, during pregnancy, a sanguineous discharge from the vagina makes its appearance at intervals which correspond to the menstrual periods of the individual, then I believe many would describe such a case as one of menstruation during pregnancy. But, whether this is *true* menstruation or not, depends upon the locality of the hæmorrhage and the character of the discharge. In *true* menstruation, the blood and epithelial debris of which the discharge consists, come from the inner wall of the uterus. A sanguineous monthly discharge during pregnancy, coming from the vaginal portion of the cervix and from the vagina, which has been ascertained to be its source in many instances,* is not *true* menstruation; it is strictly a form of *vicarious* menstruation.† Now, in what proportion of cases is a sanguineous monthly discharge during pregnancy *true* or *vicarious* menstruation?

There are no data to supply an answer to this question, and it *might* be very difficult to determine to which class a given case belonged. If we consider the question on rational grounds, it is not easy to understand how true menstruation could occur at all in a normal uterus—how an oozing could take place from many points of the inner uterine walls without hopeless detachment of the ovum or fœtal membranes. Of course, in a case of double uterus, this anatomical difficulty does not exist. As regards frequency, a certain amount of coloured discharge at or about the monthly intervals seems to be not very uncommon for the first few months of pregnancy, and probably in the majority of such cases this is vicarious menstruation as above defined; but authorities seem agreed that cases occur *rarely* in which menstruation goes on to the sixth or seventh month, or even to the end of pregnancy, the flux having all the characters of the menses—in perfect regularity—in duration, in the nature of the discharge, and also in quantity (in some cases this is said to be diminished, in others to be increased). The case of Mrs. W. belongs to this class of *typical* menstruation; and, although we cannot assert where the discharge came from in her case, it is more than probable that it came from the empty compartment of the double uterus, and that therefore this case is an instance of *true* menstruation during pregnancy. This view is supported by cases on record, to which reference will presently be made.

(*b*.) What proportion of females have a double uterus? There are no sufficient data to furnish an answer. The question can only be accurately determined by the records of

---

* Dr. A. Farre quotes Mr. Whitehead as having shown that the anterior portion of the cervix was the seat of the discharge "in all instances of supposed menstruation during pregnancy which have been investigated." *Cycloped. of Anatomy and Physiology.* "Uterus."

† Leishman, second edition, page 90.

post-mortem examinations and of the dissecting room. Now, there are but few *post-mortem* examinations in proportion to the total deaths, and of those few, there is a much smaller number in which the minuteness of examination extends to such a point as this—even in the dissecting room this abnormality might be overlooked unless purposely investigated. But, although there are no sufficient data for estimating accurately the proportion of females with double uterus, some light perhaps may be thrown on the subject by referring to the record of cases in the current literature. With this object, I have looked over the *British Medical Journal* for the last five years, from 1878 to 1883 inclusive. In this period references are met with to *ten* cases of double uterus along with double vagina, *three* cases of double uterus without any abnormality of vagina, and *two* doubtful cases of double uterus. Some of these cases were probably not observed within this period of five years, and it is possible that the same case may be referred to by different observers, and so be counted more than once (although this does not appear likely). Some deduction must therefore be made from the total number on these grounds. On the other hand, full weight should be given to the fact that, in much the largest number of the cases referred to, the uterus and vagina were both double. A double uterus is certainly much more likely to be discovered when the vagina is also double, but it does not follow that the two abnormalities occur most frequently together. I have not met with the record of a case in which the vagina was double and the uterus single, and this malformation is not among Dr. Arthur Farre's classified groups of abnormalities of the uterus.* But the converse, as we have seen, is met with and is classified—the uterus double when the vagina is single. Indeed, it may be argued that this condition is probably far more common than a combination of double uterus and vagina, inasmuch as the latter is a greater abnormality—it implies an earlier arrest of the process of fusion by which, we are told, during embryonic development, uterus and vagina, both originally double, become single organs. Among the cases of double uterus (including those of double vagina) referred to in the *British Medical Journal*, there are examples, of menstruation from both halves—of menstruation during pregnancy—of pregnancy in both compartments, and in one case, most interesting of all, ova or fœtus of different ages in the two halves. In this instance, within a few days, an embryo of one month was removed from one compartment, and one of three months from the other.* This case, if free from doubt, proves that *true* menstruation, as the accompaniment or consequence of ovulation, may occur during the course of pregnancy, and therefore that the function of the ovaries, although it may be in abeyance, is not always suspended. It may be added that the fact that miscarriage is most apt to occur at the times which correspond to menstrual periods, is another indication that the monthly congestion and consequent excitement of the generative organs recurs to a certain degree at regular intervals during pregnancy. In estimating the probable frequency of double uterus, the difficulty of recognising the abnormality must not be overlooked, and even the impossibility of being absolutely certain that a uterus is unilocular, without an actual section of the organ, must be considered.

---

* *Cyclopedia of Anatomy and Physiology.* "Uterus."

In view of all these facts, and considerations, and possibilities, we are brought, I think, to this position, that there is no evidence to prove that *true* menstruation during pregnancy is more frequent than the existence of a bilocular uterus (indeed the opposite may be the case), and, therefore, no valid argument can be founded upon *relative frequency* against the theory that true menstruation during pregnancy may be due to the existence of a double uterus.

Second.—In cases of menstruation during pregnancy have the indications of the existence of a double uterus been sought for? And, further, is it possible by examination during life to exclude with certainty the presence of this abnormal formation? The first of these questions may, I think, be answered in the negative, because, as far as I can find out, the possible connection of these two phenomena, as cause and effect, has not hitherto been specially considered. But even when searched for, the certain recognition of a double uterus can seldom be very easy during life. In Mrs. W.'s case, had it not been for the necessity that arose for artificially dilating the cervix, the discovery would not probably ever have been made. No doubt a groove† dividing the posterior wall of the body of the uterus can be easily felt in Mrs. W.'s case, and this, when present, would always be an important indication; but, in many cases it might not be easy to make a thorough examination of this situation, and even when it could be determined with certainty, that no groove or cleft existed, would one be entitled to conclude that the interior cavity was not divided into two compartments? Observations are wanting for the settlement of this question.

---

* *British Medical Journal*, 1878, vol. ii, p. 372.

† Reference is made by Dr. Arthur Farre to cases in which a "shallow furrow running along the posterior uterine wall may indicate the seat of that internal septum which more or less divides the uterine cavity into two halves, and constitutes the uterus bilocularis." *Cyclopedia of Anatomy and Physiology.* "Uterus."

Again, in Mrs. W.'s case the groove in the body was obliterated, or indistinguishable, when the seventh or eighth month of pregnancy was reached, and at the time of her last confinement, Dr. M'Ewan could not discover any of the signs that the uterus was double, although he knew it was so, and was on the alert. These considerations exhibit the difficulties which attend the diagnosis of a double uterus during life.

With regard to the general question of the connection between menstruation during pregnancy and a double uterus, Mrs. W.'s case supplies another series of facts which must not be overlooked—viz.: During the whole course of her last pregnancy, as well as during the fourth, fifth, and sixth, *menstruation did not occur.* How does this bear, it may be fairly asked, upon the theory advanced? If menstruation during pregnancy is due to a double uterus, why does it not always occur when the uterus is double? To this it may be replied that menstruation is physiologically an abnormal process during utero-gestation *under any conditions*, and therefore ought not to occur even in a double uterus, just as menstruation ought not to occur during lactation.

To sum up. In the case recorded, *typical* menstruation during pregnancy took place, and the patient is known to have a double uterus. Evidence is not at present forthcoming sufficient to decide whether *true* menstruation during pregnancy or double uterus occurs in the greatest number of women, so that this means of judging as to the possible relationship of these phenomena cannot be applied.

No case of *true* menstruation during pregnancy, as far as I am aware, is on record in which the uterus was proved *not* to have been double. This can only be *certainly* ascertained by *post-mortem* examination.

In submitting the theory that a double uterus may be the explanation of *true* menstruation during pregnancy, I do not wish to imply that it will be found to cover all cases of this phenomenon, but I think it is worthy of consideration in the discussion of a subject which is still unsettled.

CHLORATE OF POTASH AS A THERAPEUTIC AGENT.

By JOHN S. MAIN, M.D., C.M.,
Chorlton Union Hospital, Withington, Manchester.

A novel classification of the drugs of the pharmacopœia would be into those that *are* held by us in sufficient repute, and those that are *not*. Such a classification, I repeat, would be novel, perhaps Utopian, yet at the same time, as a means of drawing attention to some drugs which have undeservedly fallen into obscurity, not without its usefulness. In chlorate of potash I distinctly recognise a drug belonging to the latter class. Textbooks, as a rule, take little notice of its constitutional action, and that only in a cursory and off-hand way. It is mentioned chiefly as a topical application.

I need not take any notice of this crystalline salt *as such*, only to say, that in it we have, with the addition of strong hydrochloric acid, a ready means of obtaining chlorine gas, often very useful and convenient for purposes of minor disinfection. I would, however, before entering upon its therapeutics proper, preface a few remarks with regard to its solubility and best solvent. In textbooks the solubility of chlorate of potash in cold water is variously stated. Squire, I find, puts it at 1 in 12, Garrod at 1 in 16, and in Pereira's *Materia Medica* it is stated that 100 parts of water, at 32° F., dissolve 3·5 parts of chlorate of potash, and at 59° F. 6 parts. Now, from practical experience I would say, that all these numbers are overstated; its more correct solubility in cold water I would put as being from 1 in 20 to 1 in 24. For practical purposes, however, it is sufficient to bear in mind that one drachm of the salt will dissolve in three ounces of cold water. In hot water it dissolves readily; but unless for weak solutions this should never be used as its solvent, for if the solution be a strong one, approaching or over-reaching its solubility in cold water, it will on cooling throw out the chlorate of potash in the crystalline form, and when cooled down actually leave more chlorate of potash undissolved than if cold water alone had been used. We have, therefore, by using hot water, two evils to contend with—less of the salt permanently held in solution, and that in the crystalline form, and so with great difficulty held in suspension. The practical inference from this, therefore, is, that cold water is its best solvent, whether the solution be a strong or a weak one, provided that the salt be previously finely powdered in a mortar, this fine powder (if the mixture be a strong one) being much more easily held in suspension than the crystals by simply shaking the bottle. I may also add that the addition of dilute hydrochloric acid* assists the solubility of chlorate of potash in cold water; and at the same time liberates a small quantity of chlorine gas, which will be held in solution if the bottle be well corked.

As to its therapeutic action, this may be conveniently divided into local and constitutional.

1st. *Local.*—In aphthæ of the mouth and ulcerative stomatitis there is no more useful topical application than the glycerinum boracis of the Ph., combined with chlorate of potash. This same is also an excellent application to the fauces, by means of the probang sponge, in cases of diphtheria (acting as a sedative and antiseptic); also in those cases where the throat is too tender to admit of the use of gargles, or where gargles cannot be relied upon, as is the case with children. As a gargle in the sore throat of scarlet fever, I have found nothing more useful than a combination of chlorate of potash, biborate of soda (borax), and glycerine. This same combination, in the form of a gargle, will also be found useful in follicular tonsillitis, and all other forms of inflammation or ulceration of the tonsils or pharynx, *not specific.*† In the spongy condition of the gums of scorbutus, &c., a mouthwash of chlorate of potash, dissolved in acid infusion of roses, serves admirably.

In chronic affections of the bladder it has been recommended as an injection.

Lastly, as when given internally, it is excreted (partly at least) by the kidneys as chlorate of potash, would it be too great a stretch of the imagination to suppose that part at least of its action on the urinary tract is topical? Might not its topical action, in this way, assist in stimulating the functions of the kidneys in low fevers, diphtheria, &c., and help to explain its great usefulness in these cases? Whether this be the principle or not, I am at present strongly inclined to think that I have found chlorate of potash useful in some cases of chronic cystitis, and that alone, or combined with other recognised remedies, I think it worthy of a further trial.

2nd. *Constitutional.*—Taken internally, chlorate of potash appears in the urine as such, and not as chloride of potassium,* which in textbooks is generally stated as being fatal to the theory of its supplying oxygen to the blood. Unless it can be shown, however, that all the chlorate of potash taken appears in the urine in this way (and I am not aware that this has been done), I cannot by any means reconcile this theory with clinical experience. Its great usefulness in low fevers, and many cachectic conditions, seems to me to indicate that chlorate of potash *has* the power of yielding some life-giving element to the blood, most probably oxygen. That its presence in the blood exerts a powerful stimulating action on the mucous membranes is a point undisputed.

---

* The liquor ferri perchloridi and tinct. ferri perchloridi of the Ph. both contain a small quantity of free hydrochloric acid.

† If combined with the *appropriate constitutional treatment*, it also forms (with infusion of cinchona as a vehicle) an excellent gargle in syphilitic affections of the mouth and throat.

The following are the diseases in which it will be found most useful:—

In the first place, I would refer to its great value in diphtheria, especially if combined with the other details of treatment indicated, as given by Dr. Wade of Birmingham, these being —the protection of the entire surface of the body with flannel clothing, the administration of copious diluents to keep the kidneys in action, the administration of potassium iodide in two, three, or four-grain doses every two or three hours, and, lastly, the giving of chlorate of potash in five or ten-grain doses. If this treatment be begun early, it is often highly successful.

In the marasmus of children, given in two or three-grain doses three or four times a day, in milk, it often succeeds when nothing else will, and should always be tried. In aphthæ of the mouth, and ulcerative stomatitis, the healing action is promoted, if given internally as well as applied topically. The same holds true in non-specific ulceration of the tonsils or pharynx. Here, however, it is often useful to combine it with iron. Combined with the tincture of the perchloride of iron, it may be almost regarded as a specific in bad cases of scarlet fever. Combination with the same preparation of iron aids its efficacy in the treatment of erysipelas.

In all fevers of a low type, especially in bad forms of scarlet fever, typhus, typhoid, and diphtheria, given freely in the form of a bland drink, it is invaluable as a means of keeping the kidneys acting freely. This may be partly due to its stimulating action on the secreting cells, through its presence in the blood, and partly, as before suggested, to its local action.

In the hæmorrhagic diathesis it will also be found highly useful. Here it is generally necessary, however, to combine it with arsenic, or the tincture of the perchloride of iron, or both. In some chronic affections of the mucous membrane of the uterus it will also be found useful, alone, or combined with other remedies.

---

* A fact first pointed out by Pereira.

In anæmia, where it is our wish to improve the quality of the blood, arsenic and the preparations of iron will be found to give more satisfaction if combined with chlorate of potash. In purpura it forms a useful adjuvant to steel, arsenic, or such medicines as may be more specially indicated.

I have, lastly, to place chlorate of potash in the rank of specific remedies. That it is a specific in ptyalism was first pointed out, I believe, by Herpin, a German physician. With the aid of chlorate of potash we can cure a case of syphilis requiring mercury,* without producing salivation at all; and this is at all times an evil, and by no means wanted. In private practice especially, this fact speaks for itself, as we all know how closely salivation is connected, in the public mind, with the administration of mercury, and how the latter is detested. The chlorate of potash in these cases may be given combined with the mercury, or separately, it does not matter which. It is desirable also, when a patient is taking mercury, that the teeth and gums should be kept free from tartar. For this purpose the tooth brush should be used three or four times a day, a solution containing chlorate of potash being the tooth wash.

Having ranked chlorate of potash among specific remedies, perhaps I could not do better than leave it there. May it attract others, and so aid in approximating medicine to the exact sciences! Perhaps on nothing will this depend more than the continued discovery of specifics.

---

* In purely secondary syphilis I consider mercury a specific remedy, and the only remedy we have. The preparation I have found most useful is the bichloride, in doses of from 1-16th to 1-8th of a grain in solution. In the stage between this and the purely tertiary symptoms, a combination of mercury and iodide of potassium is indicated—either the bichloride in conjunction with potassium iodide, or what is better still, the green iodide in 1 grain doses in the form of pill, combined with 1-4th of a grain of opium if it causes gastro-intestinal irritation. For the purely tertiary symptoms potassium iodide (in doses of not less than 10 grains) is just as much a specific as mercury is in the secondaries. I would add, that this I believe to be in the main the teaching of Professor Macleod of Glasgow, and having had good opportunities of trying it, I would most humbly beg to vouch for its philosophy, practicality, and soundness.

A LECTURE INTRODUCTORY TO A COURSE ON AURAL DIAGNOSIS.

BY JAMES PATTERSON CASSELLS, M.D.

(Delivered in the Glasgow Hospital and Dispensary for the Diseases of the Ear in January 1882.)

The General Examination of the Patient.—The general examination of a case of ear disease consists of an investigation into all its stages. These are three in number—viz., *The past history, the present state,* and *the future progress* of the case.

The past history consists of an inquiry into the facts of its history up to the moment of examination.

The present state comprises a consideration of the state of the patient's health and an examination of the diseased organ.

The future progress includes a consideration of the possibilities of the case, as well as its impossibilities, under certain conditions, such as in respect to *the natural course of the case without treatment, its course under treatment,* and *its complications and consequences.*

To undertake such an examination as is here indicated, implies a knowledge of—1st. *The causes of ear diseases;* 2nd. *The mode of making a physical examination of the diseased organ;* 3rd. *The general pathology of ear diseases;* 4th. *The diagnosis of ear diseases in general;* and 5th. *Their general prognosis.*

The causes of ear diseases in general.—Ear diseases arise from the following:—

Hereditary causes.—All constitutional taints or dispositions such as are said to be transmitted to the offspring of consanguineous marriages; the syphilitic, the rheumatic, and the gouty diatheses; the scrofulous and the catarrhal constitutions, and the hæmorrhagic tendency.

Malarial causes are those that are climatic in their origin, or arise from the soil, or are the product of certain unsanitary states of the patient's own dwelling, such as marshy or clayey soils, damp and undrained houses, and sewer gas.*

Personal causes are those that are the outcome of the patient's habits, mode of life, or occupation, as obesity and the excessive use of alcohol, or tobacco, or quinine; or the habit of opium eating, or of mouth breathing;* insufficiency of sleep; violent mental emotions; sedentary or noisy occupations, and such as are obviously unhealthy in themselves, as steam loom weaving, boiler making, coal and ironstone mining, and some chemical processes.

---

* See a paper by the author, "Sewer Gas and Ear Disease," a record of struggles with foul drains and other unsanitary conditions. *Edin. Med. Jour.* April, 1878.

Traumatic causes are those that are the outcome of violence in any form, whether applied directly to the ear, as a blow or slap on the side of the head, or directly on the auricle, or to the membrana tympani, as in those who dive incautiously into the sea, or who are subjected to sudden and excessive variations of the atmospheric pressure, as in diving bells, or in the immediate presence of the unexpected discharge of a large piece of ordnance, or indirectly by *contre-coup* from blows or falls on the vertex or occiput.

The ear disease may arise from any one of the foregoing causes, or a combination of them, or from some one or several of the following states or conditions of the patient's own body, such as—

Local affections of the respiratory tract, such as acute and chronic non-specific nasal and post-nasal catarrh.

Polypous, or other tumours of the nasal passages, or of the naso-pharyngeal cavity.

Malformation of these passages, or injuries to them causing an obstruction to the free passage of the air in the act of respiration.

Cleft palate; acute and chronic tonsillitis, and hypertrophy of the tonsils; paralysis of the palate and throat muscles, or of those belonging to the Eustachian tube; affections of the mouth and ulceration of the tongue, and caries of the teeth, as well as periostitis of their fangs; some affections of the skin, as eczema and herpes. Or it may be that an ear disease as due to some one of the following diseases:—

Functional affections of the cerebro-spinal or of the sympathetic system of nerves, or of the circulatory system; of the lungs; of the stomach; of the liver and kidneys; and in uterine and ovarian irritation.

Some of the following *blood diseases* play a very important part in the causation of the diseases of the ear:—The continued fevers, as typhus and typhoid; the exanthemata, as scarlatina, measles, and variola; the mumps; diphtheria; puerperal fever; rheumatism; gout; and acquired syphilis. Any one of these diseases may give rise to a disturbance of function and tissue-change in the apparatus of hearing. Indeed, often several of them combine to make up the whole factor of causation. Thus, we may have a patient with an ear disease arising in the first instance out of some of the consequences of imperfect sanitation (malarial catarrh), whose ear after a time becomes affected in the course of an attack of scarlatina (exanthemal catarrh), and finally upon the tissue changes already caused by the malarial and scarlatinal poisons, the characteristic ones that denote the presence and action of the poison of measles are to be found. Or we may have a patient who was a subject of ear disease in early life, at which time the disease ran its course, exhausting itself, leaving, however, the organ considerably damaged. Upon this damaged condition of the tissues of the ear there may be grafted a more recent catarrhal attack of the tissues of the Eustachian tube, probably due to a chill, such as comes from a sudden lowering of the temperature of the surface of the scalp. On examination in such a case we find all the evidences of the previously active, but now exhausted tympanic ear disease, and the more recent catarrhal changes in the tube.

---

* See also a paper by the author, entitled "Shut your Mouth and Save your Life," in the *Glas. Med. Jour.* April, 1877.

The combined influences entering into the formation of the chief factor of causation in a case of ear disease, may be, and indeed often are, more complicated even than in those examples. Sometimes, too, a dulness or indifference of the patient prevents the surgeon from arriving at a knowledge of the whole of its past history, even when the patient has an intelligent appreciation of the surgeon's motives for making an inquiry into the antecedents of the case. Little points, that are of importance to the surgeon to know, are overlooked, or regarded by the patient as of no moment or importance whatever. As it is important, however, in every case of ear disease to gain a knowledge of its true cause, or causes, no practitioner ought to content himself with less information than that which enables him to form a complete mental and clinical picture of the case in all its details, in respect to its antecedent history and progress up till the moment of his connection with it.

Besides examining into the patient's family history, and his own personal life, in respect to health, habits, occupation, place of residence, and former diseases or injuries, we have to inquire into the special history of the case *as a disease*, in regard to its mode of onset and progress, and whether any and what symptoms have manifested themselves to the patient's apprehension since its commencement, and the nature of any previous treatment that the patient may have undergone. Having in this way acquired a more or less full knowledge of the *past history* of the case, we now proceed to inquire into its *present state.*

The examination of the present state of the case has for its object a physical examination of the diseased organ, and if necessary an inquiry into the present state of the patient's health, in order to compare the patient's statements with the examiner's observation of the physical facts of the case, and, when they differ from each other, to attempt a reconciliation between them.

It is always well for the practitioner, at the moment of his first interview with the patient, and before proceeding to the physical examination of the diseased ear, to endeavour to form his own opinion as to the patient's general health, judging by the general appearance, having special regard to the gait, deportment, sight, speech, voice, and visage. In this way valuable information may be obtained at a glance by an experienced eye, not only with regard to the general health of the patient at the time, but also in respect to the nature of the ear disease; thus, suppose the patient be an adult, and he enters the room with a staggering gait, and speaks in a loud unmodulated voice, you may at once suspect that he is the subject of some labyrinthine mischief, probably the affection known as "Ménière's Disease." On the other hand, if the patient be a young child, from seven to nine years of age, and comports himself as if he had some defective vision, and you find an active keratitis, or signs of its previous existence in corneal opacities, if also the teeth be scooped, or scooped and craggy as well, you may feel certain that in this particular case you have an ear disease due to hereditary syphilis to deal with. In the same way a glance suffices to tell when one has a deaf idiot child to examine, or a deaf mute who is quite sane.

As the result of some experience, I find it is better to begin the examination of a patient by the physical investigation of the state of the diseased organ, because in this way the line and direction of the inquiry about to be made into the past history of the case may be suggested more clearly to the mind of the examiner. In this way also he may see reason in making this inquiry to avoid asking certain questions, or even may deem it unnecessary to ask any at all. For example, a patient—let us suppose a married lady, accompanied by her husband—whose appearance indicates unmistakable syphilitic diathesis, comes for consultation for some defect of the hearing; if the organ of hearing is not examined at once to ascertain the cause of the defect of hearing, it is not unreasonable to say that the surgeon's mind would be prejudiced in favour of a syphilitic ear disease, and he might, in consequence, proceed to inquire into her family and personal history, even in the presence of her husband. Would this not be an imprudent step of the practitioner, especially if, when he came to examine the organs of hearing, he found the meatuses to be occluded by ceruminal accumulations, the removal of which entirely restored the patient's hearing? In such a case, had the physical examination of the affected ear been made at the outset of the interview with the patient, the state of the meatuses would have suggested to the surgeon the necessity of being silent till the *possible* cause of the defective hearing was removed, and the result of its removal observed.

Again, in this connection I think it better to let the patient relate his or her own story of the case spontaneously, as in fact all patients do come to consult one prepared to give such a statement of their case, and it is well to let them tell their story in their own way without interruption. In this manner we get much information with little trouble to ourselves, and often, indeed, we gain all that we require in this early stage of an investigation. More often, however, further inquiry is suggested to the examiner by the relation of the patient's story. Then, if we have looked at the diseased organ previously, we are in a position to make further inquiry into the case without fear of wounding the tender sensibilities of a perhaps too sensitive patient.

CURRENT TOPICS.

THE following extract from the Ballarat *Evening Post*, of 13th January, 1883, may be interesting to some of our readers. Drs. Pinnock and Ochiltree are both Glasgow graduates, and the former, by this vote, enters on his second term of three years as honorary surgeon. The poll is stated to be the largest ever known in connection with the hospital:—"At the annual meeting of the subscribers, &c., of the Ballarat Hospital, last evening, the office-bearers were elected for the ensuing year. The only offices for which any polling was necessary were those of Hon. Surgeon and Hon. Physician. For the former position Dr. Pinnock polled 251 votes against Dr. Woinarski's 44, and for the position of Hon. Medical Officer Dr. Ochiltree secured 182 votes, and Dr. Usher 113. Drs. Pinnock and Ochiltree were consequently elected to the respective offices."

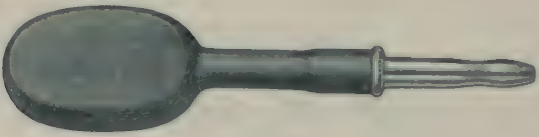
(*To the Editor*).

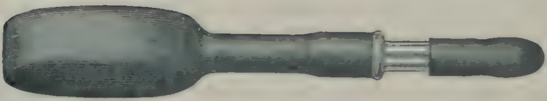
Edinburgh, *15th March, 1883.*

Sir,—In a paper by Dr. Robert Park, in the *Glasgow Medical Journal*, March, 1883, on the "Treatment of Syphilis," the author attributes to me the following remark—viz., "that no one with any great experience of syphilis now thought of treating that disease with mercury." I never made such a statement—the fact being that the non-mercurial treatment of syphilis is only adopted by a very small minority of the profession. And I fear that, so long as the ideas embodied in Dr. Park's paper—that mercury is necessary for the healing of hard chancre—hold their place, that minority will not be increased. His other remarks concerning my views are, in their way, fair criticism, and I am quite content to agree to differ from him.—I am, &c.,

FRANCIS CADELL.

Mr. Balmanno Squire's Urethral Syringe.—A full description of this instrument, and the mode of using it, may be found in the *Medical Times and Gazette* for 10th June, 1882. The syringe, which is figured about half size in the following illustration, will be found a most convenient instrument:—





The upper of the two woodcuts shows a nearly full-faced view, while the lower shows a side view. The syringe consists of an india-rubber body, from one end of which proceeds an india-rubber tube, terminated by a glass nozzle. The body is of an elliptical form, with flattened and somewhat rigid sides, and the nozzle is provided with a rubber cap, which comes off and on, so that the syringe may be filled with a supply of the solution, and be carried safely in the waistcoat pocket.

In using the syringe, it is held in the right hand, *thus*:—The fore and middle finger tips compress the syringe on one of its two flat sides, while the thumb tip rests on the centre of the other side; hence the rigid sides, when compressed, are brought into complete contact with one another at every point.

The syringe, having been previously filled, is held as above, but quite lightly, so as not to compress it. The nozzle of it is to be passed gently down the urethra as far as the glass shoulder of the nozzle will permit. Then the urethra is gently closed against the nozzle by the forefinger and thumb of the left hand. The syringe is now compressed, and then, while still compressed, is quickly but gently drawn away; at the same time, the thumb and forefinger of the left hand close the mouth of the urethra, and retain the injection for some minutes before allowing it to escape.

To fill the syringe, it is compressed (completely) as above, and the end of the nozzle immersed in the fluid to be used. The compression is then relaxed, and the syringe fills itself completely.

We have had occasion to use the syringe, and have found it very convenient, and certainly a very great improvement upon the ordinary glass instrument commonly employed.

SCHEME FOR ENCOURAGEMENT OF ORIGINAL RESEARCH IN SANITARY SCIENCE.—It is with great pleasure that we call attention to this scheme, announced by the GROCERS' COMPANY of London. The general bearings of it will be gathered from the announcement in our advertising sheet. The terms offered should be an inducement to young men of original power to devote some years to original work. There are to be three research-scholarships of an annual value of £250 each, and two of them are to be competed for in 1883 and one in 1884. The scholarships are held from year to year, but the appointments are renewable, and for the second and third years of their appointments will have the preference over new applicants. In addition to this there is a Discovery-prize to be awarded every four years. The subject for the Discovery-prize will be announced three and a half years before it is awarded, the announcement of the first being made in May, 1883. A scheme such as this cannot fail to stimulate scientific research both in this country and abroad.

REVIEWS.

A Clinical Handbook on the Diseases of Women. By W. Symington Brown, M.D. New York: William Wood & Co. 1882.

In the preface, the author of this work tells us that it does not profess to be a treatise on gynæcology. This will be readily believed when we say that the whole of the subjects usually considered, and some, such as diseases of the rectum and syphilis, not commonly taken up in works on the diseases of women, are dealt with within the compass of 247 pages. In this there is also included a dictionary of gynæcological terms, which, although extending only to two pages, contains words we do not often meet in books on this subject. Of these dyschezia, dysoötocia, endermoptosis, inopexia, metatithmenia, proctitis, and epammenorrhea may be mentioned. Those who think that gynæcology is lagging behind the other sciences ought to consult this dictionary of terms.

On page 19 we are told that limpid as well as highly concentrated urine acts as an irritant to the bladder. We are disposed to think that the frequency of micturition in hysterical patients is due more to the quantity passing into the bladder than to its being irritating on account of its limpidity. When urine irritates the bladder, it is commonly passed as soon as a small quantity has collected. A male gum elastic catheter is recommended for ordinary use. Aside from labour, we have found a silver one more easily managed, the gum elastic one being apt to bend at the point, and confuse the operator. The disadvantage of the silver instrument is that it is short, and drops of urine are apt to be spilt over the bed. This can be overcome by attaching twelve or eighteen inches of rubber tubing to the outer end.

The author gives a curious proof of the presence of inflammation of the cervix uteri. "One fact goes far to corroborate the assumption that true inflammation exists in the cervical mucous membrane; it is the accumulation of chlorides in that tissue. This can be easily demonstrated by lightly touching the surface with lunar caustic—a dense white precipitate is formed immediately. We know that in pneumonia the chlorides accumulate in the lung tissue, and are almost entirely absent from the urine, a diagnostic sign we take advantage of in that disease." We have always believed that any mucous membrane, whether inflamed or not, would turn white on the application of a nitrate of silver point, and that this is due to a normal precipitate of chloride of silver.

The following are given as the causes of anteversion:—"Anteversion may be caused by chronic endometritis, tumours in the corpus uteri, subinvolution after delivery, excessive indulgence in coitus, relaxation of the utero-sacral ligaments, shortening of the round ligaments, or by sudden pressure from above." We think that one cause not sufficiently recognised is weak health about the time of puberty. The uterus, instead of getting firm and strong, and rising from its infantile state of anteversion, remains soft, while it gets heavier as menstruation draws near, and version, if not flexion, results. The chapter on ovariotomy is very good, and is an excellent specimen of condensation. In ten pages there is given almost all that a student could be expected to know about the operation and its complications.

In treating of urethral caruncle, the following statements are made:—"Caruncles are exquisitely sensitive, and give rise to much suffering. When they occupy the urethral tube, the flow of urine is apt to be obstructed; when placed at the meatus, friction generally results in bleeding and ulceration. Pain is not always limited to the meatus, but may radiate to the back, thighs, and other parts." The important fact is not noted that one finds caruncles of all sizes and shapes with so little sensitiveness, that their presence is not known until they are seen at an examination made for some quite different purpose.

One or two Americanisms occur in the text. For instance, we are advised to blister "a surface as small as a nickel," a phrase which does not convey to a "Britisher" any very accurate idea of size. There are 39 illustrations on wood, most of them well drawn. Fig. I we take to be far from correct, inasmuch as the urethro-vaginal and vesico-vaginal septa are represented as very thick; whereas in nature there is little more than the two layers of mucous membrane and a little muscular fibre. The perineum, in its thickness, is represented as running up between the uterus and Douglas' pouch; whereas there is little depth of tissue between the upper part of the vagina and the peritoneum behind. These are the chief faults we have been able to find with the book. On the whole, it is well worth perusal, and we are not acquainted with any work containing so much sound and valuable information in so little space, and this, to the hard pressed, modern student of medicine, is a matter of no small consequence.

As a fair specimen of the matter and manner of the volume, the following extract may be given:—"Chancre. No disease has given rise to more controversy than syphilis. The celebrated John Hunter actually believed that gonorrhœa and syphilis were identical. Many medical books, published within fifty years, contain the most astonishing jumble of nonsense about venereal diseases. Robert Druitt, in his *Surgeon's Vade Mecum* (1841), evidently confounds chancroids with chancres. He says—'If a chancre last for a few days only, there will be no fear of secondary symptoms.' It is only during the present generation that clear ideas have been formed as to the essential distinctions between gonorrhœa, chancroid, and chancre; while many points relating to hereditary syphilis still remain undecided.

"At present, few surgeons deny that a simple chancroid is more than a local affection, and is never followed by constitutional syphilis; and nobody believes in the identity of the gonorrhœal and syphilitic poisons.

"True syphilis has been compared to variola or scarlatina. Like them it has a period of incubation, and one attack generally shields the individual during her lifetime. In some persons the effect produced on the system dies out, and they are liable to be infected a second time on exposure. The period of primary incubation lasts from two to six weeks—generally not more than four weeks.

"The ulcer in most cases is quite superficial—a simple erosion—and therefore likely to be overlooked in women. Its most frequent seat is one of the labia majora, or minora. But any part of the mucous membrane may be affected. Chancres have been found in the urethra, the anus, high up in the vagina, and even on the cervix uteri. The nipple may be affected from suckling a syphilitic infant. Or the disease may be communicated to the lip during the act of kissing, Generally the base of the sore is more or less indurated, like parchment; more rarely it resembles a split pea. There is no inflammation or soreness. In this respect it differs from a chancroid, which is always painful. After a variable length of time induration disappears. The secretion is mostly serum, unless the sore is irritated by caustics or dirt, when pus will be formed. If the superficial ulcer is allowed to run its natural course, no cicatrix will remain after it heals.

"Chancres are single, not multiple, unless the virus is communicated to several abraded surfaces simultaneously. For, although the virus of chancre is contagious when applied to a person who has never had syphilis, it is not auto-inoculable.

"One of the most characteristic early symptoms is the induration of the neighbouring ganglia, sometimes called syphilitic bubo. As a chancre occurs most frequently on the genitals, the glands in the groin are commonly affected; the induration takes place within a few days after the ulcer appears, rarely later than a week. If the chancre is situated on the lip, the submaxillary glands become indurated. There are no signs of inflammation present; and this symptom is apt to be overlooked, especially in fat patients. The induration is likely to last for several weeks, it may be for many months. A syphilitic bubo rarely suppurates, the exceptions occurring in scrofulous or broken down women, and not very often even in them. A chancroidal bubo often ends in suppuration, so that the formation of pus in a bubo is corroborative evidence that the original disease was not syphilitic."

Legal Medicine. Part I. By CHARLES MEYMOTT TIDY, M.B., F.C.S. London: Smith, Elder, & Co. 1882.

FIVE years ago a ponderous volume, entitled *A Handy Book of Forensic Medicine and Toxicology*, was published as the joint production of the late Dr. W. Woodman and Dr. Meymott Tidy. Notwithstanding its size, the encyclopædic nature of its contents, and the immense labour which had evidently been expended upon it, that book did not claim to be a complete treatise on the subject. The very handsome volume, recently published by the surviving author of the *Handy Book*, under the new title of *Legal Medicine*, does, however, claim to be the first instalment of a complete treatise. We are not astonished at the feeling of "solemn responsibilty" with which Dr. Meymott Tidy informs us he has undertaken this task, but for a different reason from that which he puts forward. Medical jurisprudence is a subject which is so wide in its scope, that a complete treatise would require a more or less extensive discussion of the whole circle of the physical sciences. We cannot but admire the courage and industry of the man who ventures upon the task of collecting, "from English and foreign literature, all recorded cases having reference to the various subjects under consideration," and for the purpose of "clearing up what was ambiguous and reconciling what was contradictory," proposes to "institute new inquiries and conduct fresh experiments in most of the subjects." If Dr. Tidy adheres to this ambitious programme, we shall ultimately have a series of volumes in bulk and number somewhat like a modern encyclopædia, the size of which will seem to most readers as out of all proportion either to the importance or to the necessities of the subject.

Part I is an octavo volume of 600 pages, for which the publishers have done everything to satisfy the most fastidious taste in type, paper, and binding. The author begins with an introductory lecture, as delivered to the students of the London Hospital Medical College, on the process of law and medical evidence. In style this lecture is somewhat florid, but it contains much sound advice couched in forcible language. In Chapter II, the mode of expression alters, the professional "I" being changed for the editorial "We," which is maintained throughout the rest of the volume. In this chapter, the signs of death, the various changes which the tissues of the dead body undergo until putrefaction completes its disintegration, and the other important medico-legal questions which may arise in connexion with the dead body are fully discussed. This chapter is exceedingly complete and exhaustive, and the plan which the author adopts here, and carries out in every other part of the work—viz., giving the illustrative cases together at the end of the chapter, and referring the reader to each case as it seems to bear upon important statements in the text by a numeral, is an exceedingly good one. In this way the reader, at a first reading, gets a much clearer and more comprehensive view of the subject in hand, without having his mind disturbed by long and detailed histories of cases. The diligence and thoroughness with which the author has perused the literature of the subject is proved by the long list of cases culled from English and foreign sources, which are thus narrated in considerable detail at the end of each chapter. Although most of these cases are to be found in the other standard works on the subject, the fact that the author has in each case consulted the original report, and corrected errors that had crept into published records, makes this part of the work of great value. The only adverse criticism which we have to offer to this part of the volume is, that there is a certain amount of unnecessary padding here and there, which the author would do well to omit in future editions of his work. For instance, in speaking of the entire and continuous cessation of the heart's action as a sign of death, the following paragraph appears:—"The noise produced by the heart under ordinary conditions of health may be resolved into two sounds, commonly represented by the syllables lūbb—dŭp." He then goes on to describe the rhythm of the heart's action, and to tell us about the changes of these sounds in valvular disease. Such information has no direct bearing on the point in question, and is not needed by the readers of Dr. Tidy's book.

Again, in treating of the effects of extreme cold, and the medico-legal questions which may arise out of the criminal exposure of children to the inclemencies of the weather, he digresses into a discussion of the hygienic management of childhood, and among other things informs us that "the sleeping room for children should never be below 60° F., nor the living room below 65° F." Again, in connection with the subject of starvation, we have a dissertation on the varieties of foods and their respective values, which might be quite suitable for a work on physiology or hygiene, but has no necessary or important bearing upon the department of legal medicine to which Dr. Tidy devotes himself. While in one direction our author unnecessarily increases the bulk of his volume by unnecessary details, in another direction he commits the opposite error. In speaking of the medico-legal aspects of age, he does not note the difference between English law and Scotch law in matters in which the difference is clearly defined and important—thus, p. 162, "If a rape with consent be committed on a girl between the ages of ten and twelve, the crime is regarded as a misdemeanour." In Scotland, such a crime would be a felony, even if the girl did solicit the criminal. We hope, when Dr. Tidy comes to treat of criminal offences in detail, he will point out the differences which exist in the law of the two countries, and so make his book of more value to his readers on this side of the border. Again, in the chapter on hair and fibres, we miss the very admirable plates showing the microscopic characters of the hairs of different animals which are to be found in the *Handy Book*, to which we are referred in this volume. In a complete treatise, we do not think that it should be necessary to refer to what is practically a previous edition of the same work for such illustrations. In addition to the subjects which we have mentioned, the author discusses very fully the various medico-legal aspects of sex, including monstrosities and hermaphrodism, expectation of life in its relation to life assurance, burns, lightning, and explosives. The space at our disposal forbids us from entering upon any extended criticism of the manner in which these subjects are treated. While in some of the articles we have details of observations and experiments by the author which are interesting, we do not observe anything very new or original as a result of the "new enquiries" and "fresh experiments *de novo*" to which he refers in his preface.

We are glad, however, to note that on every subject he has been careful to incorporate all that is important in recent medico-legal literature. It is no disparagement to a work on medical jurisprudence, to say of it as has been said of Dr. Tidy's present volume, that it is largely a compilation from the works of other standard authors. As a work of reference, we can highly recommend this volume to our readers. It will form a very valuable addition to the libraries of medical and legal practitioners.

A Dictionary of Medicine, including General Pathology, General Therapeutics, Hygiene, and the Diseases peculiar to Women and Children. Pp. 1,816. Edited by RICHARD QUAIN, M.D., F.R.S. London: Longmans, Green & Co. 1882.

THE aim of this work, as stated in the preface, is to bring together, in a form to allow of ready and easy reference, the latest and most complete information as to scientific and practical medicine, the facts and observations in regard to which are diffusely scattered in journals, monographs, scientific treatises, and transactions of learned societies. It is not, however, to be regarded as only a dictionary, "but also as a treatise on systematic medicine, in which the articles on the more important subjects constitute monographs in themselves, whilst definitions and descriptions of matters having less claim to extended notice, are given as fully as is required. Thus, an endeavour has been made to supply, in a clear, condensed, and readily accessible form, all the information that is at present available for the use of the practitioner of medicine." With such a scope, the volume is necessarily large and unwieldy, and the type disagreeably small; but these drawbacks are compensated for by the excellence of the substance.

In its production, Dr. Quain has been assisted by a staff of about 160 writers, including Lockhart Clarke, Spencer Cobbold, Matthews Duncan, Sir Joseph Fayrer, Sir William Jenner, Charles Murchison, Sir James Paget, &c., while the majority are men whose names are favourably known to the profession. We are inclined to think, however, that this number is unnecessarily large, when we note that such intimately related subjects as Typhus and Typhoid Fevers are by different writers, and that Inflammation, Hyperæmia, and Cirrhosis of the Liver have been committed to three separate hands. Possibly this has arisen to some extent from the fact that many of those who were Dr. Quain's colleagues when the work was commenced did not live to see the conclusion of his labours, their places being taken by other men. In this way we may account for the absence of Murchison's signature under the various articles on the continued Fevers and on diseases of the Liver, to which we referred in the hope that we might still find something from his valued pen. The only article by him that we have seen is that on Jaundice, and it is such as to cause regret that he did not live to contribute more largely to the pages before us.

With such a numerous band of writers, it is only to be expected that there should be various degrees of excellence, literary and otherwise. But it may be said in general that the method adopted, and the careful editorial supervision, have combined to produce a tersely and clearly written work, which will take a valued place in the practitioner's library. It is somewhat difficult at times to see the principle on which space has been allotted for the various articles; thus, Gout gets 15 pages, while Kidney Diseases, including the articles on Bright's Disease and Albuminuria, are dismissed in 20. When under the heading Typhus we find no description of the course of the temperatures, and an unqualified statement to the effect that "the amount of rash is of little moment, and is no guide to the severity of the attack," we are inclined to wish that a little more space had been allowed, so as to admit of a fuller description, and of less dogmatic statement of opinions contrary to those held by men of the largest experience in fevers. Many of the articles are simply brief definitions, usually with references to general articles where the subjects are discussed in detail. But among these we look in vain for Crossed Paralysis, Alternate Paralysis, Anthracosis, Decubitus (in the sense of Acute Bedsore), Parovarian Cyst, &c.

A special feature of the work is the giving of the derivations of words, and this part seems to us to have been hastily done and imperfectly revised. For instance, we find the following:—"Lymph (νύμφη, a nymph, water);" "Lymphangeitis (νύμφη, water or lymph, and ἀγγεῖον, a vessel);" "Lymphangiectasis (*lympha*, lymph, and *angiectasis*, vascular dilatation);" "Adenoma (ἀδήν, a gland, and ὁμός, like);" "Erysipelas (ἐρύω, I draw, and πέλας, near)." It is impossible to account for such a mistake as νύμφη, but *angiectasis* may be supposed to refer to an article given in the dictionary as *angiectasia*. In the case of *adenoma*, an attempt has been made to account for the termination, but in deriving sarcoma, carcinoma, and other similar words, this attempt has wisely been abandoned, as it fails to account for the fact that Greek words with that termination end in *ωμα*. All that can be said about it is, that the termination has been adopted to signify a tumour. With regard to *erysipelas*, a reference to Liddell & Scott would have shown the writer that there occurs in early Greek medical writings the word ἐρυσίπελας, which is derived from the root of ἐρυθρός, red, and πέλλα, the skin.

We know that the publication of such a work must have involved enormous labour, and details, when comparatively unimportant, are apt to receive but slight attention. It is but right, however, that in succeeding editions such mistakes should be remedied; and from its nature, the work is one whose value can be maintained only by frequent revision and extension. Frequent editions will undoubtedly be called for, as the volume in large measure comes up to the expectations that had been formed of it, and will no doubt commend itself to students, but especially to practitioners, as meeting a want in the shape of a ready work of reference of moderate dimensions, and up to date.

The Relative Mortality after Amputations of Large and Small Hospitals. By HENRY C. BURDETT. London: J. & A. Churchill. 1882.

THIS is a reprint of a paper read by Mr. Burdett before the Statistical Society, and is the result of inquiries which he sent to 160 cottage hospitals. Answers were received from ninety-two, into thirty-one of which no cases requiring amputation had been received. The amputations under review were performed in sixty-one hospitals, from their institution until the end of the year 1878, a period of twenty years. The total number is 326, the deaths 58, and the mortality 17 per cent. To prevent his statistics being impugned, as Sir James Simpson's were, Mr. Burdett gives details of the cases, and has only collected actual figures, which could be definitely verified from the books kept by the medical staff of the different hospitals. He also gives the causes of death so far as could be ascertained, and these include pyæmia and tetanus, formerly the scourges of our great hospitals. For purposes of comparison, Mr. Burdett is content to take the statistics of Sir James Simpson, published so long ago as in 1869, and those of Mr. Erichsen in 1874, of the large Metropolitan and Provincial Hospitals: but we submit that he ought also, in fairness, to have brought down the statistics of these to the end of 1878, as, owing to the introduction of antiseptics and other causes, the mortality in them has been considerably reduced. To give an example: the mortality after amputation in the Glasgow Royal Infirmary, as given by Sir James Simpson in 1869, was 39·1 per cent; but in this *Journal*, in the year 1877, Dr. Thomas, the superintendent, showed that the mortality to the end of 1873 had been reduced to 32 per cent, and still later, in the *Lancet* of last year he gave the mortality for the previous eight years as 22·8 per cent, or a decrease of nearly 42 per cent in the space of twelve years. This is a remarkable decrease, and we have no doubt a similar result has been obtained in other hospitals. The mortality in the large German hospitals used to be very great, but the rate given by Dr. Schede, of Hamburg, of cases treated in them antiseptically, is only 2·9 per cent. This is a result which we fear will never be attained here, owing to the severity of the injuries admitted into our hospitals; but, still, we have seen that progress has been made, which we trust will be continued. Mr. Burdett is eloquent in his praise of the antiseptic system, and indicates that a peerage should be given to Mr. Lister and Spencer Wells for what they have done towards the saving of life. We quite agree that honours should be bestowed on these and others, as they are very sparingly dealt out to eminent members of our profession.

He likewise admits that Listerism has solved the question of large and small hospitals, and declares that their size and condition are of comparative unimportance, so long as they are kept antiseptic. With this we quite agree, but at the same time are of opinion that cottage hospitals should be introduced into some of the populous places in the neighbourhood of Glasgow, from which many severe cases of accidents are taken to our infirmaries, as the nearer the hospital is the better for the patient.

The discussion which followed the reading of the paper at the Statistical Society is also given. We were amused at the remarks of one speaker, who gravely told his hearers that "the mortality in the Glasgow hospital in which Mr. Lister himself practised, was so great that the building had to be pulled down and a new one erected." Not one of those present corrected him; and we only trust that Mr. Moore and the other members of the Society are better posted up in statistics than they are about the Glasgow hospital. With this exception we commend the book to the notice of our readers as one from which much valuable statistical information may be gleaned.

Since writing the above, we have received the annual report of the Glasgow Royal Infirmary for the year 1882. The number of amputations performed was 93, the deaths 15, and the mortality 16·1 per cent. Deducting ten amputations of the hand, ankle (Pirogoff), foot, and a re-amputation of the leg, the number is 83, with 14 deaths, or 16·8 per cent; whilst for the four major amputations only, it is 16·3 per cent: results better than those shown in the Cottage Hospital statistics.

The Essentials of Bandaging, with Directions for Managing Fractures and Dislocations; for Administering Ether and Chloroform, and for Using other Surgical Apparatus; and containing a chapter on Surgical Landmarks. Illustrated. By BERKELEY HILL, M.B. Lond., F.R.C.S. Fifth edition. London: Smith, Elder & Co. 1883.

THE issue of a fifth edition of such a work is the best proof of its value and fitness for the purpose it professes to serve, and it is particularly and unusually gratifying to find that the improvements, or at least additions, inevitable in new editions have not in this case materially increased the bulk of the volume. Too often do we find, especially in books of this character, handbooks to a special section of professional work, that the first useful and condensed manual is, in its later editions, expanded into a more or less systematic treatise, and so ceases to serve its primary purpose, yet does not advantageously take the place of works originally more pretentious.

Viewed as a whole, the book is worthy of the highest commendation; some parts of it, indeed, are, as far as we know, unique in their utility. Nowhere in such a terse, clear, and practical way are the "surface guides and landmarks" laid down, and for the more advanced surgeon this will probably form the most useful chapter, enabling him at a moment's notice to refresh his memory as to the exact guides for any procedure—a piece of surgical knowledge which we have always found one of the most difficult to retain in the mind with accuracy.

There are a few faults, chiefly of omission, to be detected, most of them arising from the purely metropolitan methods recommended. Especially do we remark the absence of many arrangements and appliances familiarly used in all Scotch hospitals. Indeed, what is given as the Scotch long splint is perhaps the most unsatisfactory description in the book. We venture to say that a long splint put on exactly as directed could not be found in any Scotch hospital to-day. The section on the administration of anæsthetics seems to us decidedly meagre, and to some extent misleading. The bald statement that "the pulse gives the earliest indication of syncope, it should therefore be constantly under the finger," is directly contrary to the teaching of some distinguished surgeons; and the extreme importance of very forcible drawing forward of the tongue when any respiratory failure occurs—not to remove the tongue from the glottis, but by traction on the parts at its root, to excite by reflex action respiratory movements—is not made sufficiently clear. Again, the different effects of ether and chloroform in the abolition of eye reflex is not noted. The avoidance of stimulant before the administration of chloroform is enforced, although it is directly contrary to the physiological indications and to the practice of a very large number of surgeons. The whole section on anæsthetics might, in the next edition, which will doubtless very soon be required, be with advantage made more complete, and if space for this is needed, it might be obtained by the omission of the paragraphs upon the laryngoscope and ophthalmoscope, instruments the use of which cannot possibly be learned from any book, and which are of far too great importance to be explained to advantage in the few lines devoted to them here.

Amongst minor details we fail to find any mention of Martin's bandages, or of the dilating bags for plugging the nares which, when obtainable, must supersede all other appliances for this purpose. While making the above remarks rather as suggestions than as criticisms, we feel that their fewness in number and small importance show how great is the value and how excellent the method and manner of execution of the work. We feel sure that this edition will prove as acceptable and useful to its readers as the previous editions have long been.

Annual Report of the Board of Regents of the Smithsonian Institution, showing the Operations, Expenditures, and Condition of the Institution for the Year 1878. Washington: Government Printing Office. 1879.

The same for 1879 and 1880.

In our issue for February, 1880, we directed the attention of our readers to the Smithsonian Report for 1877, and took occasion to explain the origin and nature of the Smithsonian Institution. We have now before us the three succeeding volumes of these Reports, and again make them the subject of a short notice, partly by way of due acknowledgment of our obligations to the trustees for transmitting them, and partly to keep them in the remembrance of our readers. It is impossible to review these volumes either analytically or critically. The contents are so multifarious in their nature, consisting in about equal proportions of an index of the work of the Institution, and of original and translated or abstracted papers, mainly on anthropological, physiological, and natural historical subjects, that an analysis such as our space would permit would consist of nothing more than a series of titles, with names of authors. Criticism is also impossible, the scope of the matter is so great and its subject so various. We can only say that as an index for research, especially in anthropology and zoology, there is no series of books which the student will find so valuable and indispensable for reference as these Annual Reports. They contain many original papers of great importance, and they guide to many more, both in the voluminous publications of the Institution, and in the literature of all languages. We may specify as illustrative of the truth of this general expression of opinion, that in the volume for 1880, the last which has come to hand, there is a Record of Scientific Progress, by competent authors, in these departments—Astronomy, Geology, Physics, Chemistry, Mineralogy, Botany, Zoology, and Anthropology. Among the Miscellaneous Papers there are a Bibliography of Anthropology, extending to about forty pages of small type; a Report on the Luray Cavern, Virginia, with several illustrations; a Discussion, with diagrams, of Prof. Snell's twenty-five years' Barometric Observations; an original Investigation of Illuminating Materials, reprinted from the Report of the U.S. Lighthouse Board for 1875; a Synopsis of the Scientific Writings of William Herschell, occupying above one hundred closely printed pages, ending with a "Subject Index" alphabetically arranged, with the date and reference to the original publication in each case; and lastly, Reports of Astronomical Observatories in America and elsewhere, describing the *personnel*, the instruments, and the character of the observations made. This volume is probably richer in material than any preceding; but all will be found worthy of reference by the student in almost every department of scientific research. The country which possesses an active and liberally managed agency for the collection and generous redistribution of knowledge, such as the Smithsonian Institution, is to be envied and congratulated. When the ample resources from which the great expenditure incidental to a work of such world-wide utility is derived, are, as in this case, provided, not from the revenue of a nation, but from the beneficent endowment of a humble citizen, our envy is stimulated and our congratulation made all the heartier.

Regional Surgery, including Surgical Diagnosis; A Manual for the Use of Students. Part I—The Head and Neck. By F. A. SOUTHAM, M.A., M.B.Oxon., F.R.C.S.Eng. London: J. & A. Churchill. 1882.

IN describing this work as "including surgical diagnosis," the author fails to give an accurate idea of the limits of it, as in actual fact it "includes surgical diagnosis" and *nothing else.* A more correct title would have been "The Diagnosis of the Surgical Affections of the Head and Neck," as we have herein no word on the etiology, pathology, or treatment of the diseases mentioned. While it is true that many diseases have a tendency to affect special localities, there are so many others which are liable to occur in any and every part of the body, that a work carried out on a purely regional method must be marked by a great deal of rather wearisome repetition. Mr. Southam has endeavoured to avoid this by the use of reference numbers, which refer the reader to the paragraph wherein the disease is first described. In this way the difficulty has been partly overcome, but with the result of making the work very tedious reading, so as to be adapted only to the peculiar stamp of mind which takes delight in working out the intricacies of *Bradshaw's Railway Guide* and other similar compilations. Every page, however, bears evidence of wide reading and careful investigation, and we know of no work which contains within small compass so accurate a description of the surgical affections of this region; the rarest as well as the most common receiving their due share of attention. The adoption in so many instances of the tabular form, has enabled the author to place in distinct contrast those diseases having many features in common, and in this way we find the differential diagnosis of rodent ulcer, epithelioma, and lupus, of syphilitic and epitheliomatous ulcers of the tongue, and of the different forms of stricture of the œsophagus, displayed in a manner easy to be understood and remembered.

We think the publication of the book in three volumes, appearing at intervals, will seriously prejudice its sale, as few care to purchase the first volume of a work without some assurance of being able to obtain the last; we suppose the purpose in publishing part of a book in this way is to occupy the field and so exclude competitors, but it seldom secures this result, and often so limits the sale as to discourage both author and publisher from proceeding further.

The Owens College Course of Elementary Biology. Part I.—*The Frog: an Introduction to Anatomy and Histology.* By A. Milnes Marshall, M.D., D.Sc., Fellow of St. John's College, Cambridge, &c., &c. London: Smith, Elder & Co. Manchester: J. E. Cornish. 1882.

This book is the first instalment of a guide to the practical work in the course of elementary biology at Owens College. It consists of a histological introduction and an anatomical description suitable for a handbook on the dissection of the frog. We are not aware what previous training the students of zoology in Owens College have undergone, but certainly any teacher who has students capable of working through the dissections, &c., described in this book is to be envied. Of the anatomical part we can only say that it seems absolutely exhaustive of the subject, and arranged with a precision and method which leave nothing to be desired. Especially is the printing to be commended, as, by a judicious use of various kinds of type, an astonishing amount of lucidity is obtained. Of the histological portion, or rather, the histological methods described, we regret that we cannot express approval. They are not only meagre but confused, and in some instances mistaken. For example, it is advised to stain tissues in mass before cutting sections—a method which we certainly never saw tried, and which, on the face of it, does not seem likely to give good results. If the piece is small enough to stain it must be smaller than is advantageous for cutting. The simple plan of removing air from specimens by immersion in boiled water or salt solution is not given, while other cumbrous methods are. Even in the otherwise excellent anatomical and histological portion microscopic methods seem a stumbling block; *e.g.*, the student is advised to study the circulation of the blood in the web of the frog's foot—no mention being made of the other situations in which it can be more advantageously observed.

The anatomical and histological part of the book is admirable, and leaves practically nothing to be desired. It is a pity that the wholly unnecessary introduction to histological methods was not replaced by a mere reference to some of the numerous excellent textbooks already existing upon this subject.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM DR. CAMERON'S WARDS.

COMPOUND COMMINUTED FRACTURE OF SKULL, WITH RIGHT HEMIPLEGIA—[Reported by Norman Maclehose, M.B., House Surgeon.] J. L., æt. 18, was admitted to Ward XX, on 18th November, suffering from a severe injury of the skull, caused by a string of iron "washers" falling from a considerable height upon his head. He was stunned by the blow and fell to the ground unconscious; but when admitted to hospital, to which he was brought almost at once, he was clear and coherent, though in a state of tolerably deep shock. On examination, it was found that the wound was situated over the left parietal bone, and consisted of a laceration of the scalp, combined with a compound comminuted fracture of the skull. The fracture is in the form of a hole punched out of the bone, so that the pulsations of the brain could be distinctly seen. The rough edges of the fractured bone were felt round the greater part of the aperture, and there did not appear to be any depression, nor, on introducing the finger, were any loose spicules of bone to be felt. The wound was carefully washed out with solution of carbolic acid, and one stitch put in the scalp, free drainage being provided for below. Antiseptic dressings were applied, the head having previously been shaved for a considerable distance round the injured part. As regards the nervous symptoms, it was found that there was profound motor paralysis of the right arm and partial paralysis of the right leg. There appeared, so far as the dull perception of the patient permitted one to judge, to be no sensory paralysis of either arm or leg.

21st November.—It is noted that the paralysis of the right arm remains as profound as ever. That of the right leg has in some measure disappeared. On testing the arm with an interrupted galvanic battery, the muscles respond freely to a feeble current.

25th November.—The patient is much as before, so far as the power of his limbs is concerned. The wound is not aseptic, there being a discharge of slightly fœtid pus. Frontal headache is occasionally complained of, but this usually disappears after free purging.

30th November.—About five o'clock P.M., on the day before yesterday, the patient, who since last report has been making satisfactory progress, called the nurse's attention to a twitching of the muscles of the right half of the face. For a day or two previous to this, complaints had been made of slight headache in the left frontal and temporal regions. These contractions of the facial muscles lasted from fifteen to twenty minutes, with one or two short intermissions. Since then, patient has progressed much as before, but there is present an unmistakable, although slight, right facial palsy, there being a distinct deviation of the tongue to the right side, while there is an evident expressionless condition of the right side of the face. He can pucker up his lips sufficiently to whistle, but with difficulty. On dressing the wound one or two long hairs were observed protruding, and pulled out of it; and on further search some small tufts of hair, evidently carried in between the fragments at the time of the injury, and a loose comminution of some size, were removed. In other respects the wound appears to be in a satisfactory state. The right arm remains *in statu quo.* The temperature keeps normal, and the pulse is regular, though slow, usually numbering about 48 beats per minute.

3rd December.—There has been no return of the twitching movements of the face, and the facial paralysis is gradually passing off, that of the right leg having in great measure disappeared. The right arm is still powerless, and is now beginning to atrophy; so much so, that, at a point three inches below the elbow, the left arm measures half-an-inch more in its circumference than the right.

7th December.—The wound is healing slowly, and there is no fresh incident to report in the case.

11th December.—To-day it was noticed that the patient could move his right arm very slightly. The muscles most under his control were those of the upper arm.

14th December.—This return of power is gradually increasing, and is evidently travelling from above downwards. He is as yet unable to move his fingers, but he can raise his arm above his head, and very slightly flex and extend the forearm, but is quite unable to exert any pressure with the fingers or thumb.

17th December.—He can now bend his fingers a little, and move his arm generally more than before. The facial paralysis has entirely disappeared, and the wound itself is doing well.

CASE OF MALIGNANT PERIOSTITIS AND PYÆMIA WITHOUT EXTERNAL WOUND.—M. P., æt. 15, was admitted to hospital on 12th November, 1882, complaining of pain and swelling over the left hip, with inability to move the left leg. The patient was in a state of high fever when admitted to the surgical wards, and had been equally so for two days previously in Ward IX, where she was first received as a medical case.

She cannot give any cause for her illness, but says that while at her work, which was that of carrying heavy pieces of clay at a brickfield, she was constantly in the habit of resting her hand on her left hip and bringing weight to bear on that part. Nine days before admission she was suddenly seized with severe pain in the left gluteal region, and this increased so much as to prohibit at first walking, and latterly even movement of the limb.

On examination, which could not be made complete owing to the patient's feverish and excitable state, it was found that pain was distinct over the left hip, and also, though less acutely, in the right knee joint. On careful manipulation over the left gluteal region, deep fluctuation was made out. Gentle passive movement of the limb did not cause so much pain as direct pressure over the swollen area. When gentle rotation of the leg was practised, the head of the bone moved in the joint easily, and without any roughness or grating. The girl herself was much emaciated, the lips were pale and the cheeks white.

On the following day she continued much in the same state (except that swelling over the hip was more pronounced, and fluctuation more distinct) till the afternoon, when she complained of pain in the side and difficulty of breathing. There was great thirst and high fever, the temperature being above 103°. On auscultation loud friction was heard all over the chest, and also in the cardiac area. Jacket poultices were applied and quinine administered, but the symptoms did not abate.

Next day the swelling had increased over the hip and in the left iliac region, and in both situations fluctuation could now be distinctly felt. On consultation, it was deemed unwise to open the abscess in the critical condition of the patient. Friction was as general and as loud as before, and the breathing laboured and shallow. Temperature still above 103°. During forenoon she had been conscious, but so weak as to be unable to speak; towards afternoon, however, delirium set in, and she died about three P.M.

The *post-mortem* examination was made by Dr. Coats, of which the following is a note. The body is emaciated; and on the skin there are a few scattered petechial spots and pustules. The heart is normal in size, but in its wall a pale area is found which may be an abscess. There is a considerable quantity of fluid in the pericardial sac.

The lungs contain a large number of metastatic abscesses, but mostly of small size. The areas affected are always distinctly demarcated and have generally a pale colour, but sometimes they are red in the central parts and pale peripherally.

The kidneys present a number of very minute white spots. The left is enlarged, weighing 6 oz.; the right weighs only 4½ oz. The liver is somewhat enlarged, weighing 3¼ lbs., and only two small white spots are discovered in it. No abscesses are discovered in the spleen. The broad ligament in the neighbourhood of the left iliac fossa is thickened and somewhat adherent, but no purulent exudation is noticed.

The left wing of the iliac bone is denuded of periosteum, both in its external and internal surfaces, and in each case there is a considerable cavity filled with pus. The pus also infiltrates largely the connective tissue on the left side of the pelvis, especially in the posterior parts, but it does not extend higher than the level of the first lumbar vertebra. No pus is discovered in any of the veins, although the infiltration of the connective tissue is such as to render it impossible to state whether any of the finer veins contain pus or not.

MEETINGS OF SOCIETIES.

GLASGOW MEDICO-CHIRURGICAL SOCIETY.

Session 1882–83.

Meeting IV.—12th January, 1883.

Dr. Alexander Robertson *in the Chair.*

Dr. A. Scott, Tollcross, was elected a member.

Dr. James Whitson read (1) case of adenoid sarcoma of the mamma, and showed microscopic sections of the tumour. Lantern slides from these were shown on a screen, and a communication was read from Mr. Adolf Schulze, who made the photo-micrographs, detailing the methods adopted in their production. (2) CASE OF COMPOUND FRACTURE OF THE CLAVICLE; and (3) CASE OF RADICAL CURE OF HERNIA. In the two last cases the patients were shown.

Dr. Macewen said that he had never seen a tumour of the kind of such enormous size as that exhibited. The results of the operation had been most excellent. In regard to the second case he had never seen a compound fracture of the clavicle. The result of the operation had been very good, with comparatively little thickening. The operation performed in the third case had been pretty frequently resorted to within the last four or five years. Mr. Wood, who has performed the operation many times, admitted that there was a tendency to the return of the hernia. In one case he (Dr. Macewen) had operated on, the hernia was double, and the tissues extremely lax. The ring was not distinct; the abdominal walls were so lax that the whole hand could be introduced through the opening. After a couple of operations he got the parts into a condition that a belt could be worn. In Dr. Whitson's case there was some impulse on coughing, and he would advise that a belt should be used, as the connective tissue was apt to stretch. To Mr. Schulze they were greatly indebted for the photo-micrographs exhibited. For teaching purposes they had great advantages over microscopic specimens.

Mr. Maylard said that the case was not one of adenoid sarcoma, but of adeno-sarcoma. In reference to the last case of hernia, the use of the wire would be apt to set up a good deal of irritation, and would interfere with the process of healing. In the present case it would be wise to wear a belt.

Dr. Barlow said that it appeared to him very doubtful policy to leave the wire to eat its way out, as suggested by Dr. Whitson. The process of "eating out" would be a very slow one; and in view of the fact that the wire got loosened it could be easily removed. The case of hernia exhibited showed a slight impulse, and it would be premature to pronounce it cured.

Dr. Hugh Thomson said that a very excellent splint for fractures of the humerus was that which used to be known as *croix de fer*. Its effect in pressing back the shoulder was such as no other apparatus could bring about. In the ordinary bandages for keeping back the shoulder it was forgotten that, where the clavicle was broken, the pole or stay which threw the shoulder back was absent; and that the effect of directly pressing it back was the very reverse, that is, to throw it out.

Dr. Newman described the tumour of the breast in its pathological aspects as a typical adeno-sarcoma, and through all its parts of the same character.

Dr. Whitson, in reply to what had been said by Dr. Barlow, said that in regard to the removal of the wire in the clavicle case, it was a question whether, from the very considerable force necessary for its removal, it was better to leave it to work its way out than to run the risk of refracturing the bone.

MEETING V.—2ND FEBRUARY, 1883.

DR. GAIRNDER, *President, in the Chair.*

MR. W. H. S. WALKER, Sick Children's Hospital, was elected a member.

DR. WOOD SMITH read NOTES OF A CASE OF CATALEPSY, and showed the patient.

The President said that no doubt many of those present had for the first time that evening seen a case of catalepsy. In the course of a long experience he had himself met a good number of cases which, in one or more points, approached the peculiarities of catalepsy; but he had never seen a perfectly typical case, as this was. If the condition in which the patient at present was came to be prolonged, probably a lunatic asylum would be a more proper place for him than a general hospital. In regard to former cases not so treated, it was strongly impressed on his mind that some of them had been allowed to die of starvation. In an asylum the risk of this termination of the case was greatly diminished.

Dr. Alexander Robertson said that in a pretty extensive experience of nervous diseases he had never seen a case resembling the present. The condition generally corresponded to that of those cases seen in asylums, and classed as "melancholia with stupor," or "acute dementia." There was the same complete abeyance of intellectual activity as in acute dementia. In the latter there was often a general torpor of the mental powers, coldness and blueness of the extremities, and sometimes a want of sensibility in regard to position—the patient standing in the same position for a considerable period. One essential point of difference between acute dementia and catalepsy was the plastic rigidity exhibited in the latter. The causation of both conditions was much the same—strong religious excitement being sometimes a factor in the causation of melancholia with stupor. In regard to intensity of expression there was a resemblance between the two conditions. In cases of melancholia there seemed to be present an overpowering delusion—the person perhaps believing that he was acting under the direct commands of the Deity. Dr. Smith remarked that, on being pricked, no bleeding followed in this case. In this fact there appeared to lie a valuable suggestion as to treatment. It was evident that there existed some contraction of the arterioles and of the capillary vessels. This indicated an affection of the nerves, connected with the circulatory system; and might there not exist a similar condition of the nervous system generally? Might there not be a state of contraction of the small vessels supplying the nervous centres? and if there were, their function would be materially interfered with. In convulsions from bleeding there was a deficiency of blood supply to the nerve centres; and in epilepsy the same condition existed. But instead of a tonic contraction they might suppose that the condition in this case was gradual. This would produce a certain rigidity of the muscular system, and the small arteries would be diminished in calibre. This indication seemed to suggest for treatment some agent fitted to relax these vessels. Possibly the exhibition of the nitrite of amyl by inhalation might have a good effect, as its action in many cases was to relax the small vessels. But if the case continued for much longer time, undoubtedly a lunatic asylum was the proper place for the patient.

Dr. Finlayson said that he had seen several cases which more or less nearly approached to catalepsy, in connection with hysterical attacks. But before this case he had seen only one marked case of typical catalepsy. It was in the Edinburgh Royal Infirmary; and he only saw it casually. In that case—that of a woman—the cataleptic seizure came on suddenly, and then suddenly passed off. In the course of the short time that he was present, she had three separate attacks. During the attacks she was insensible, and her limbs could be placed and would remain in any position, even one in which, under ordinary conditions, she could not have remained but for the shortest period. In a short time the spasm relaxed, and she woke up to her ordinary state. In the present case the condition was of a chronic character, and did not pass on into attacks of an acute kind. Then, again, in this condition this man was capable of undertaking and executing movements when ordered to do so. The remaining point of peculiarity in this case was the sex of the patient. Catalepsy in a man was certainly very uncommon.

Dr. Fergus referred to a case which he had seen many years ago in the hospital of a London workhouse. In that case the patient, a charwoman, was able to follow her usual employment during the intervals between the attacks.

Mr. Maylard asked whether the optic discs had been examined, and if so, the condition; and also whether tendon reflex was present or absent?

Dr. Wood Smith said that the eyes had not been examined, and that tendon reflex was absent.

Dr. W. L. Reid mentioned a case in which attacks of a cataleptic kind came on at the menstrual periods. The patient would remain in the most awkward positions for a considerable time. The present case, had it happened in a woman, would have been associated with hysteria. Its occurrence in a male showed that it was not hysterical in the true sense.

The President said that, many years ago, he had seen in the north of England a young lady, nearly related to a distinguished statesman, in a condition in which she had been fed for some months with the stomach pump. She lay in bed, her limbs quite flaccid, constantly muttering, but in no other sense in a state of coma. She ultimately got well, and he believed was still alive.

MR. WILLIAM J. FLEMING showed a SUBSTITUTE FOR THE JURY-MAST IN THE TREATMENT OF SPINAL DISEASE. A description of the apparatus will shortly appear in this *Journal*. Dr. Fleming made a demonstration of its application; and a short discussion followed in respect to its mechanics as a means of lightening the weight on the vertebræ.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.

SESSION 1882–83.

MEETING IV.—TUESDAY, 9TH JANUARY, 1882.

The President, PROFESSOR M'CALL ANDERSON, *in the Chair.*

DR. CRAWFORD RENTON showed the KNEE-JOINT AND SIX INCHES OF THE FEMUR, removed from a patient ten days ago on account of ulceration of the cartilages of the joint and osteo-myelitis of the femur, which was much thickened. For these reasons amputation through the middle of the femur was performed. Previous to operation, the patient had rigors and pain, and swelling of various joints, but since the limb was removed, she has daily improved, and is now free from pain in any joint.

DR. BEATSON showed a patient in whom he had EXCISED THE ENTIRE PATELLA for disease of that bone. He also exhibited the bone itself, which presented all the characters of a typical patella. The boy had made an uninterrupted recovery, and as a considerable time had elapsed since the performance of the operation, there could be no doubt of the excellence of the result. He could now flex and extend the limb perfectly, and was able to run, walk, and use his leg in every way.

DR. CAMERON showed a man, 35 years of age, on whom he had operated on account of a TRANSVERSE FRACTURE OF THE PATELLA, which had united by a wide ligamentous union. The fragments before the operation were nearly three inches apart when the limb was fully extended, and so far apart during flexion of the joint as to admit the breadth of the hand between them. The patient had no power of extending the limb, so that in walking with the leg perfectly straight, if by chance the foot caught on some irregularity of surface, so as to cause a little forced flexion, he fell forwards. He was therefore compelled to wear an apparatus which prevented the possibility of flexion. In May last Dr. Cameron operated, with antiseptic precautions, by wiring the fragments and putting a drainage tube into the joint. The wire (thick silver wire) was cut short and left in. The result is very satisfactory. The man now walks about with hardly any lameness, and has no inclination to fall. His only difficulty is in coming down stairs. He can extend the limb vigorously, but he cannot flex it fully, owing to the great shortening of the extensor apparatus.

Dr. Dunlop had seen the case before Dr. Cameron, and had thought that it was almost impossible to improve it. He had great pleasure in congratulating Dr. Cameron upon his result.

Dr. M'Ewen thought that the result in this case was excellent. He had a case where there were four or five inches between the fragments, with an inch of shortening of the ligament as in Dr. Cameron's case. He tried Malgaigne's hooks. He then made four or six V shaped cuts in the muscles, and pulled till he got the fragments together, and wired them in the same way. The case did well. He did not know that it was good to leave the wires, as they were apt to produce irritation—not chemical irritation, but that which was likely to arise from movement of the parts.

Dr. Crawford Renton had seen Dr. Beatson's case along with him. The whole knee-joint was exposed in the operation. His friend, Dr. Bruce of Dingwall, had attempted to get union by using pegs; but on removing them, the parts had stretched again, so that he thought Dr. Cameron was right in using wires.

Dr. Fleming wished to say a word about leaving the wires *in situ*. He had seen a case under Lister, where he caused a wire to slowly cut through bone by attaching a piece of india-rubber to it, and keeping up continuous, but slight tension. In Dr. Cameron's case, he thought that there was some risk of this taking place.

Dr. Cameron, in reply, said that Dr. M'Ewen had shown there was no difficulty or risk in making V shaped incisions in the muscles. If any danger were arising from the wire by its presence setting up undue irritation, it could be removed any day in a very short time. With regard to Dr. Fleming's remark it must be remembered that the tension in Lister's case was kept up day and night. He had been much struck with the excellent result in Dr. Beatson's case, which certainly was a very remarkable one; and it showed that no patella at all, was better than a bad one, such as that seen in a badly united fracture.

DR. ROBERT POLLOK exhibited an OVARIAN CYST, which was described by Dr. Newman as a unilocular one, with smaller cysts embedded in or studded over the interior. Attached to the cyst was a body which might be the ovary, but as it had undergone cystic degeneration, it was impossible to say. There was no evidence of the presence of Fallopian tubes. The cyst had been removed by Dr. George Buchanan from an unmarried lady, 32 years of age. Five years ago the same patient was operated upon by the same gentleman, and a cyst removed, which was diagnosed as ovarian by Dr. Finlayson, Dr. Buchanan, and himself. The recovery was good. Menstruation had been regular throughout the growth of the cyst, and was re-established about fourteen days after operation. About a year ago, pain and distinct abdominal enlargement reappeared, and a second cyst (specimen exhibited) was removed on the 16th October last. On both occasions the cysts were favourable for operation, being free from adhesions. The pedicle was treated by the intra-peritoneal method, and no drainage, spray, or special Listerian treatment was adopted. The abdominal incision healed by first intention. Menstruation appeared in the second instance on the tenth day after the operation. The special interest of the case was the fact (if this was really a case of double ovariotomy) of the continued menstruation. The fluid, which unfortunately had not been chemically examined, was in the first operation more limpid and less gelatinous than in the second, and the first cyst was smaller. From these facts, he looked upon the first as a parovarian, and the second only as an ovarian cyst, and considered that there was still one ovary remaining to carry on the important function, although some might regard this as an instance of the non-essentiality of the ovaries in the production of menstruation.

SIMPLE PERFORATING ULCER OF THE ŒSOPHAGUS OPENING INTO THE LEFT BRONCHUS AND CAUSING GANGRENE OF THE LUNG.

Dr. Finlayson, in showing this specimen, said it was interesting as an illustration of the simple perforating ulcer of the œsophagus, exactly resembling the kind found in the stomach. The occurrence of such simple ulcers had been denied by some, but there seemed good evidence of their being found in the parts of the digestive tract adjoining the stomach—viz., in the lower part of the œsophagus, and in the duodenum. He referred to a case by Professor Flower, in the *Medico-Chirurgical Transactions* for 1853, vol. xxxvi; in this case the perforation took place into the aorta; the ulcer there described resembled exactly a simple ulcer of the stomach; references to various authorities for the occurrence of such ulcers in the œsophagus are given in this paper. Since then not a few cases have been recorded. It was quite true, however, that most of the perforating ulcers of the œsophagus were due either to the destructive lesions of malignant disease or to the effect of mechanical irritants, such as bones and pins, or to the influence of both—as the retention of some irritating substance above a malignant stricture. In a slight search of the literature of the subject, Dr. Finlayson had found several cases more or less resembling the present, and he referred specially to a case communicated by Mr. Part in the eighth volume of the *Pathological Transactions*, where a simple ulceration of the œsophagus had penetrated the right bronchus, and had led to destruction of a part of the right lung. He also referred to an interesting case communicated by Dr. Wilks, and seen by Mr. Hilton (*Pathological Transactions*, vol. vi), where the diagnosis of a communication between the œsophagus and trachea was made by means of the œsophageal tube, as air was found to pass up the tube during respiration. The present case presented appearances in the lung such as are occasionally seen to result from foreign bodies retained in the bronchi, and Dr. Joseph Coats, who made the examination, was struck by the resemblance of the lung in this case to the appearance presented in a case, formerly communicated by him to this Society, where a prolonged illness, mistaken for ordinary phthisis, had resulted from the impaction of a mutton bone in the bronchus. In making the present examination, he searched the bronchi for any foreign body, but the explanation was only reached when the œsophagus was removed and slit open; the foreign body searched for was then plainly seen to consist of portions of the food passing into the left bronchus and sucked up by the respiration into the left lung, which was hollowed out into numerous cavities filled with gangrenous matter. The right lung presented evidence of what seemed an impending affection of the same kind, although it had not gone the length of breaking down. The clinical facts of the case were as follows:—The young man, 22 years of age, had been healthy, and presented in his family history no indications of phthisical disease. Three months before admission he had vomited his food, without any apparent cause, and this was repeated frequently for a week or two, by which time a cough began to come on. No blood had been brought up at first, but for some weeks before admission blood was frequently present in the matters vomited or expectorated. On admission he was extremely ill, much wasted, and presented the appearance of advanced phthisis. The expectoration and breath were very fœtid and had a gangrenous odour. The physical signs revealed consolidation and great excavation in the left side, but little affection of the right side being recognisable. The temperatures were always high. The prominent feature was the combination of vomiting and coughing, and during the few days he lived in the hospital no satisfactory explanation could be arrived at regarding this: it was supposed that the case was essentially one of phthisis, with some gangrenous complication, and that the vomiting and coughing might be due to irritation of the larynx during the swallowing of food. It should be stated, however, that the patient was not hoarse, that the larynx was not examined during life, and that after death it was found free from disease. During the week he was in the hospital, the facts referred to were commented on by Dr. Finlayson as very peculiar, and different from anything he had seen. The patient sat up in bed to take his food, and he always secured a basin by his side. After taking a mouthful or two of fluid, he began to cough and vomit and spit, the contents of the basin being partly vomited food, partly dirty pus with a very offensive smell, and often an intimate admixture of blood with the pus. He was quite ready to take his food, but seemed to vomit up, in the process of swallowing it, about one half of what he took. The cough likewise occurred apart from the taking of food. In view of the *post-mortem* examination this condition became quite intelligible, although very puzzling during life.

DR. NEWMAN showed MICROSCOPIC SECTIONS OF THE MYOMA OF THE UTERUS shown at last meeting by Dr. Renton.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

Prolapse of Rectum treated by Hypodermic Injections of Ergotine.—Dr. Jette writes favourably of Vidal's treatment of this affection. He says that Bonjean's or Yvon's ergotine only should be used. The former may be employed diluted to a sixth of its usual strength with cherry-laurel water, 15 drops representing 80 ctgr. of ergot, which is about the dose which it is prudent to commence with. The injection must be made as near as possible to the part which it is desired to influence, the needle being introduced about 5 millimetres from the anal orifice, parallel to the intestinal wall, and should be pushed in to the depth of 2 to 4 ctm., that is, into the fibres of the sphincter. The injection should be made slowly, to avoid causing pain, which is always considerable. The treatment may last from a few days to several weeks; and to protect against relapse 3 or 4 supplementary injections should be made after apparent cure. M. Jette's success in using this method of treatment has been invariable, and in those of his cases seen eight or ten months after there was no relapse.—(*Thèse de Paris*, 1882.) *Bull. Gén. de Thérap.* 28th February, 1883.

Phenoresorcine.—M. F. Reverdin offers this as a new antiseptic which possesses many advantages over most others.

He prepares it by mixing 67 parts of carbolic acid with 33 of resorcine. This yields a product which crystallizes slowly. 10 per cent of water added to it dissolves it and keeps it fluid; and this mixes with water in all proportions—one respect in which this combination is superior to simple carbolic acid.—*Journal de Thérap.* 10th March, 1883.

Mixed Anæsthesia.—M. P. Aubert, after referring favourably to the use of morphia hypodermically as an adjuvant to chloroform inhalation, writes even more highly of morphia and ether similarly used; but his best results in mixed anæsthesia are obtained from the combined use of morphia, atropia, and ether. The morphia renders the action of the anæsthetic more rapid, and permits the induction of unconsciousness with a smaller dose; while the atropine diminishes the excitability of the pneumo-gastric nerve, and thus gives almost absolute security against sudden arrest of the heart's action. The solution employed by Aubert is the following:—

Hydrochlorate of Morphia, . .	10 centig.
Neutral Sulphate of Atropia, . .	4 milligr.
Distilled Water, ...	10 gram.

Of this he injects about a gramme and a half, that is, 12-15 milligrammes (about $\frac{1}{5}$ grain) of morphia and 6-7 tenths of a milligramme (about $\frac{1}{100}$ grain) of atropine. This is injected twenty-five to thirty minutes before administering the ether. Anæsthesia results in three to seven minutes; the ether is well borne, and gives rise to none of the signs of the usual stage of excitation; there is no vomiting during anæsthesia; the return to consciousness is easy and calm. Those who have practised this method of mixed anæsthesia have been struck with the difference between it and ordinary anæsthesia with ether, the former silent and quiet, the latter attended generally by initial excitement, ready vomiting, and disturbed awakening. The great superiority of ether over chloroform lies in its well known harmlessness; its defects are, that it is more disagreeable to patients, and produces insensibility more slowly. But as the preliminary injection of morphia and atropine renders the action of ether as rapid and as pleasant as that of chloroform, why not always use ether in this way?—*Lyon Méd.* 14th January, 1883.

A Victim's Account of Cinchonism.—Dr. Kempf (Indiana) gives the following account:—"I once suffered an attack of acute tonsillitis, and to prevent suppuration I concluded to cinchonise myself thoroughly. When I took the first dose of

10 grains of quinine, my temperature was 104° F. One hour afterwards I was restless, could not sleep, and a peculiar condition of the nerves of my body, causing trembling, was noticed. These symptoms were probably due to cerebral congestion. I now took another dose of 10 grains in capsules, and lay on a bed with my head buried in the pillow, waiting for further developments. I was acutely awake, and sensitive in the highest degree. A scissors fell on the floor in the next room, and I imagined that it fell on my body point foremost, causing an electric shock. Gradually a sense of fulness in the head came on, accompanied by a feeling as if a band tied round my head was slowly being tightened. Then, away in the distance I heard something which reminded me of the March winds approaching. Louder and louder they grew, and nearer and nearer they came. All at once they struck the house as if they were going to turn it topsy-turvy. I raised my head to take a last look at the wall, but a sense of giddiness and vertigo overcame me, and I sank back. Presently the door opened, and a kind voice asked something, and I yelled back at the voice to talk louder, as I was almost deaf. A louder voice asks—'Do you want something?' 'I want to be let alone,' I answered back, and tried to gulp down my angry feelings, but the swelled tonsils gave me a warning not to try it again. I was in about as miserable a fix as I could wish for. My temperature was 102° F. three hours after the second dose, pulse about 80, and an intense headache slowly approaching threatened to make things dreadful. A vomiting spell capped the climax, and after two hours of intense wretchedness, I fell into a sleep, made restless by terrorising dreams. The sequels were a diarrhœa accompanied by griping pains, and a feeling in my head for a day or two as if its contents were trying to escape. I had been thoroughly cinchonised."—*Louisville Medical Herald.* May, 1882.—G. S. M.

Rheumatic Œdema and Supra-clavicular Pseudo-lipoma.—Prof. Potain, along with M. Verneuil, has described a singular affection, which he has called pseudo-lipoma of the supra-clavicular region, and which appears to be connected with the rheumatic diathesis. Ovoid and triangular, this swelling is contained within the borders of the supra-clavicular fossa; it is ill defined to the eye and to the hand; it is elastic, and does not retain the impression of the fingers. This affection is not painful, and may last for several years without giving any trouble. No surgical operation is required, but it is very interesting from a diagnostic point of view.

All the patients of M. Verneuil, as also those of M. Potain, were rheumatic. In the majority there was tumefaction, chronic and painful, of one or more articulations. The urine presented a marked tendency to the formation of gravel. But in one patient M. Potain determined the complete absence of signs of rheumatism or gout.

The pseudo-lipoma is always connected with chronic rheumatic affections; among M. Potain's cases two were diabetic, and one had atheromatous dilatation of the aorta. A good number of the cases suffered from abnormal irritability of the nervous system.

Dampness did not seem to take any part in the etiology. In all cases in which M. Potain was able to determine hereditary antecedents, it was on the maternal side. The etiology of the affection would be complete if one could discover under what influence the pseudo-lipoma is produced.

This affection sometimes extends beyond the region which has been assigned to it, invading the adjacent and other parts of the body. Often one finds, in individuals attacked with chronic rheumatism, a tumefaction of the supra-clavicular region, and of the back of the hand. The hand is almost always an arthritic one, especially in females.

In several cases the pseudo-lipoma became considerably enlarged and troublesome in cold, damp seasons.

A certain number of the patients have, at the same time, some œdema of the subcutaneous cellular tissue. M. Potain observed this eight times in his twenty cases.

One cannot separate the supra-clavicular pseudo-lipoma from this mobile œdema, between which there are all gradations. One must admit in these cases a serous infiltration, in spite of the difference in consistency. This œdema of the arthritic is not, moreover, an isolated fact; without the pseudo-lipoma œdema is often found in the rheumatic without other cause than the rheumatic diathesis. M. Kirmison, in a memoir inspired by M. Gayon, and M. Davaine have noted acute temporary œdemas found in the course of rheumatism. Three conditions may contribute to give rise to this œdema—the action of damp cold, disturbance of the nervous system, and some local affections which may provoke an acute, sympathetic œdema.

A temporary œdema may then show itself in the course of acute or chronic rheumatism.—*La France Médicale.* 19th October, 1882.—G. S. M.

Absorption of Sequestra.—From experiments, in which pieces of dead bone and of ivory were inserted into the medullary cavity of the tibia of rabbits, M. Lannelongue has come to the following conclusions:—

1. That the absorption of sequestra is a fact which has been experimentally demonstrated.
2. That suppuration in the cavity holding the sequestrum is an obstacle to absorption.
3. That absorption of dead bone takes place more rapidly and easily than of ivory; besides, it is followed by the formation of new bone.
4. That bone employed for this purpose must be carefully disinfected.—*La France Médicale.* 20th May, 1882.—G. S. M.

Primitive Laryngeal Cancer.—The *Lyon Médical* for 19th November last, contains a review of an essay on the Symptomatology of Primitive Cancer of the Larynx by L. Desprez.

By primitive cancer he means that which originates in the vocal cords, and which may afterwards extend to the epiglottis and arytenoid cartilages. Since the publication of the thesis of Blanc in 1872, he points out that many cases have been recorded, and he estimates the number as being over one hundred and fifty, the principal observers being Ziemssen, Faurel, Krishaber, Koch, Descants, and Morolle.

Cancer of the larynx is rare in females, because they possess other organs more predisposed to it. M. Desprez agrees with Krishaber that there is no diathesis, and that the majority of the sufferers are robust, and have no family tendency toward the disease. The two principal types are epithelioma and encephaloid. In the encephaloid form hæmorrhage frequently occurs, the tumour being very vascular, and appearing first as a little bud. In the epithelioma, on the contrary, the disease steadily involves the tissues.

He advocates tracheotomy, but makes no mention of total extirpation of the larynx.—J. A. A.

Sudden Death during Labour.—Dr. G. Heinricius, of Helsingfors, relates the particulars of a sudden death during labour. The patient was a washerwoman, 38 years of age, in sound health so far as known, and with her third child. She walked into the hospital in the afternoon, and on examination was found in labour, a first cranial position, os uteri dilating, and pains strong. Within an hour after her admission she suddenly became convulsed, and died in a second or two. She was immediately seen by the doctor, and within five minutes after the death of the mother the child was brought away by the forceps, and after forty minutes' treatment, began to breathe freely and cry, and is doing well.

The *post-mortem* showed that about half an inch above the aortic valves, the two inner coats of the aorta were ruptured. The external coat had been dissected up and the pericardium opened into by the stream of blood. The blood-vessels and organs generally were found healthy. The rupture was considered to be due to severe straining during a pain.—*Centralblatt für Gyn.* 6th January, 1833.—W. L. R.

Germs and the Spray. By JOHN DUNCAN, Surgeon to the Royal Infirmary, Edinburgh.—Is the spray efficient as a protection when wounds are uncovered during an operation or the changing of a dressing? Mr. Duncan instituted a series of experiments to test this. Flasks containing pure fluid were brought near a wound while the spray was acting, some of them being inoculated with discharge, and it was found that the latter as often remained pure as those without the discharge. Thirty-seven flasks were exposed with aseptic fluid without the spray, and an equal number with it, and the result was that nine of the former and seven of the latter putrified. The time of exposure to the air and spray varied from one minute to forty-five minutes, and no successful example was obtained after exposure for twenty-five. These experiments seem to prove that so far as destruction of the floating germ in the air is concerned, the spray is perfectly ineffectual.—*Edin. Medical Journal.* March, 1883.—J. C. R.

Sequelæ of Tracheotomy Occurring after Closure of the Tracheal Wound. By J. MAXWELL ROSS, M.B., Edinburgh.—In this paper Mr. Ross points out that in many cases where tracheotomy has been performed there remains a certain amount of hoarseness, and at times attacks of dyspnœa are experienced.

In the case which forms the text of his paper, a small diaphragmatic membrane existed opposite a tracheal wound, which had been made in infancy for the extraction of a foreign body.

In other cases polypiform vegetations have been noted, and so troublesome have they been that tracheotomy has had to be again performed and the growth removed. At times, after tracheotomy had been performed, considerable trouble arose with regard to removing the tube, and the cause of this was found to be due to granulations which had sprung up around the tracheal opening, and at times on the posterior wall. As regards the prevention of the above sequelæ, surgeons are still undecided whether abandoning the canula and resorting to tubage of the larynx, or adopting a soft form of tracheal tube, or giving them up entirely, would be best.—*Edin. Med. Journal.* March, 1883.—J. C. R.

The Frequent Repetition of Doses.—In a lecture on this topic, lately delivered by Prof. A. A. Smith, in the Bellevue Hospital Medical College of New York, the speaker mentioned a variety of facts, of general interest, respecting the action of drugs when thus administered.*

The following are, in brief, some of the suggestions offered:—

Chlorate of Potassium.—Grain doses, given every half-hour, in scarlet fever, diphtheria, tonsillitis, &c., will produce the same results as larger doses, without the danger of the evil effects resulting from the accumulation of the drug in the system, as sometimes happens when it is administered in the ordinary way. Indeed, he believes that they will produce better results upon the throat inflammations.

Croton Chloral, when used to relieve neuralgia, is more efficacious when a grain is administered every half-hour until the symptoms abate, than when, as is usually the case, five to eight grains are repeated every two hours until fifteen grains are taken. Another advantage of the former plan is that it avoids the irritation of the stomach so apt to be caused when the larger doses are used.

Salicylate of Sodium.—Two grains given every half-hour or hour will often quickly relieve obstinate cases of urticaria, not chronic in character.

Balsam of Copaiva, given in drop doses every half-hour, is another excellent remedy for urticaria, although not so often successful as the salicylate of sodium.

Solution of Arsenite of Potassium (Fowler's Solution).—Half a drop, given every half-hour for six or eight doses, will often relieve the vomiting caused by a debauch, as well as the morning vomiting of drunkards, and the sympathetic nausea and vomiting of pregnant women.

Jaborandi, given in five to ten minim doses of the fluid extract, will produce diaphoresis, without depressing the heart's action; an effect which has so often followed the use of larger doses as to cause many physicians to refrain from their use, especially in the puerperal state.

Sulphate of Atropine.—One hundredth of a grain, dissolved in a glassful of water, and given in teaspoonful doses every half-hour or hour, is a valuable remedy in spasmodic croup. An emetic or purgative should precede it when the condition of the stomach or bowels demands it.

---

* *N. Y. Med. Jour.*, 10th Feb., 1883.

Bromide of Sodium.—A few grains, dissolved in a tumblerful of water so that each teaspoonful may represent a half-grain, will quickly quiet the nervous disturbance of teething infants, or fever not dependent upon the onset of an inflammation or other grave trouble, but rather such as may follow excitement of any kind. The dose should be repeated every ten or fifteen minutes.

Ipecac.—A drop of the wine of ipecac. repeated every ten or fifteen minutes, will often relieve obstinate vomiting from different causes, and will sometimes also check diarrhœa. Given in this way in water it does not nauseate.

Calomel, according to Trousseau, given in the sixtieth of a grain, every hour, for ten or twelve hours, will stop nocturnal headache due to syphilis. Professor Smith has verified this statement with the use of one-fortieth of a grain. Children who regurgitate food after nursing, may be relieved by giving every ten or fifteen minutes a teaspoonful of a grain of calomel in a pint of water. In order to dissolve it, the calomel should first be put into an ounce of lime water, and this be afterwards added to the pint of pure water.

Mercury with Chalk.—One twenty-fourth of a grain, given at intervals of fifteen to twenty minutes, is often of great benefit in the vomiting and non-inflammatory diarrhœa of children.

Corrosive Sublimate.—One grain in a quart of water. Of this a teaspoonful every hour will be of benefit in diarrhœa with passages containing mucus and indicative of inflammation.

Tartar Emetic.—Put one grain into one quart of water. Give of this teaspoonful doses every half-hour, and it will relieve the wheezing and cough accompanying a slight bronchitis in children.

Nux Vomica.—A drop of the tincture, given every ten minutes, will often produce most marked relief in sick-headache not of neurotic origin. It should be given immediately or soon after meals.

Cantharides.—A single drop of the tincture, given every hour, will in many cases relieve vesical catarrh.

Digitalis.—A single drop of the tincture, given to a patient suffering from symptoms due to heart disease where digitalis is indicated, administered at intervals of an hour, or half-hour, according to the severity of the symptoms, will often give greater relief than larger doses, and without liability to ill effects.

Castor Oil.—For the diarrhœa of children, accompanied with slight inflammation, straining, and the passage of jelly-like matter, but not true dysentery, five drops of castor oil, given every hour with sugar and gum, is an excellent remedy.

Pulsatilla.—Two-minim doses of the tincture, repeated every hour, are said by an authority in venereal diseases to have given greater and more rapid relief, in cases of orchitis and epididymitis, than any other mode of treatment. Professor Smith also vouches for its efficacy when administered in this manner, in dysmenorrhœa not of a membranous, obstructive, or neuralgic character.

Calabar Bean.—One-fiftieth of a grain of the extract, repeated every half-hour, for six or eight doses, is an effectual remedy for the flatulence, and a sensation of palpitation or fluttering at the pit of the stomach, suffered by many women at the menopause. After the number of doses above mentioned have been taken, three hours should intervene before recommencing its use.

Ergot.—Minim doses given every half-hour, for six hours on the day before the menstrual flow should recur, and again on the day when it is due, will be beneficial in amenorrhœa not dependent on anæmia. Quite remarkable, although contradictory to the above, is the efficacy of the same plan of treatment when used to control excessive menstruation.

Aconite.—One-third to one-half minim, given every fifteen minutes, will lessen fever (when it is not the commencement of one of the continued fevers), when the skin is hot and dry, the pulse full and bounding, and the mucous membrane of the throat and nose dry. When so used, it will also arrest a "cold in the head," and is useful in cardiac hypertrophy, with palpitation, severe headache, and disturbances of the nervous system, due to increased force of the heart-beat.

Hamamelis.—Two minims, given every half-hour, will often control hæmorrhage. In spite of previous doubts, Professor Smith has been convinced of its beneficial effect in hæmorrhage from the nose, uterus, and from hæmorrhoids.

Belladonna.—Minim doses, given every half-hour, are a good remedy in cases of nasal catarrh and bronchitis, accompanied with free secretion. Owing to its tendency to cumulative effects, it should be suspended for a time after eight or ten doses have been so given. Thus administered, it retards the exudation of serum, and sustains the heart's action, in pulmonary œdema.

Chloride of Ammonium.—Two grains, combined with ten or fifteen minims of the tincture of cubebs, given every half-hour, oftentimes controls acute pharyngitis, and superficial inflammations of other tissues about the throat. When the pharyngitis depends upon a gouty diathesis, add to the above mixture ten minims of the ammoniated tincture of guaiacum, and administer every hour.

Citrate of Caffeine.—One grain, every half-hour, often produces most marked relief in migraine.

Gelsemium.—Three minim doses of the tincture, every half-hour, will often relieve miraculously neuralgias about the face or head, and leave no ill effects.

Guarana.—Fifteen minim doses of the fluid extract, given every fifteen minutes, frequently relieve headaches, especially when they are periodical and not of malarial origin.

The Relations of Tubercle and Inflammation.—M. Kiéner records his ideas on this subject, supporting himself by an investigation recently undertaken along with M. Poulet upon tuberculous ostitis or caries of the bones. He first reviews the anatomical relations of tubercle of the bone and inflammation; in the primitive and chronic form, the tubercle does not provoke inflammation; the follicles, sharply circumscribed, develop separately, and the surrounding tissues only present lesions of impaired nutrition. They do not cause more reaction than the fibrous tubercles at the apex of the lungs. The indifference of the tubercle is much less in the second stage, the follicles are not so well circumscribed; the action of the infectious principle oversteps their limits, as is testified by the numerous giant cells disseminated at the edges; suppuration, although partial and chronic, is the rule. It is especially in the form which may be called acute tubercular ostitis, which recalls by its progress the galloping consumption of the lungs, that the relations of tubercle and inflammation are closest. In this grave form the tubercular follicles, while but imperfectly defined, are almost immediately destroyed by suppuration, before caseation; in this case the inflammatory reaction predominates.

The second part of this communication is confined to the etiological relations of tubercle and inflammation; the preceding facts show that the primitive tubercle, in spite of its virulence, may be tolerated for some time in a healthy organism; inflammation then is not a direct consequence of the action of the virus; it may be connected either with the diminution of resistance of the economy or of the tissues, resulting from hereditary predisposition or any debilitating cause, or with a malady foreign to the tubercle and acting in concert with it. The influence of wounds, catarrhal affections, and of suppurations is studied from this point of view.—*Gazette des Hôpitaux.* 30th January, 1883.—J. L. S.

Treatment of Cancer of the Uterus.—We make the following extracts from a very interesting and readable clinical lecture on "Cancer of the Uterus," by Prof. W. Goodell, author of *Lessons in Gynecology.* The case under discussion was that of a woman aged 37, a multipara, who suffered from menorrhagia, bleeding after coition, and a foul-smelling discharge. As regards physical examination, he says:—"Passing my finger into the vagina, I come upon a sore which is characteristic. It is crater-like. There is a hard, irregular margin surrounding an excavation, which has on its bottom and sides friable granulations. This is typical of carcinoma.

"The examination that I made has caused a little bleeding. That is one reason why you should never use a speculum in these cases. The finger tells the whole story, and a speculum may cause a hæmorrhage difficult to control.

"What about the prognosis? It is very unfavourable. Out of all the cases on which I have operated, and of which I know the after results, only three have I considered cured. Still, I can prolong life, and that is a great thing. In some of the cases on which I have operated, the disease has not returned in the cervix, but in some other part of the body. I have operated on women apparently in the last stages of the disease, so low that you would not give them two weeks' lease of life, and have seen them get out of bed and live for over two years. My experience is, that the older the woman the more likely is the operation to be followed by success. In younger women there is more blood in the part, there is a luxuriance of growth, and they are not so apt to be benefited by an operation."

In reference to treatment Goodell says that Freund's operation is rarely permissible, and then only when the womb is freely movable. "The operation which I shall perform to-day will consist in scraping away the cancerous matter as far as possible, and trying to reach healthy structures. The removal of the friable granulations will arrest the bleeding, which may not return. In doing this, I shall use this serrated curette, and this fenestrated forceps. In buying a fenestrated forceps, you should get it with the obstetrical lock, so that you can fasten the blades securely together. I am removing a large quantity of this offensive material, and my fingers are going to smell very badly. How shall I get rid of it? I shall first wash them well with soap and water, and then with turpentine, which is very useful under these circumstances. Then I shall again use soap and water, with another go with the turpentine. After this I shall probably immerse them in carbolised water. Permanganate of potassium is an excellent disinfectant, but it has the disadvantage of so staining the hands that one is not presentable for several days after its use.

"Now, suppose a woman comes to you and you diagnose cancer of the uterus, are you going to say, 'Madam, I am very sorry to tell you that you have a cancer?' No, don't you do that. I should not tell even if she asked me to tell her the truth; but in the majority of cases they do not want to know, and will say to you, 'Now, doctor, if you find a cancer, don't tell me.' No matter how good a woman is, or how fully prepared for the future she may be, the knowledge that she has a cancer is a terrible blow, and she at once gives up, begins to go down hill rapidly, and soon dies. I never, except in very rare instances, tell the patient that she has a cancer; but I always tell some member of the family, or a friend, exactly what is the matter. Suppose the patient asks straight up and down, 'Is it a cancer?' You do not want to tell a lie, and you do not want to say that it is a cancer. I get out of it in this way: I say, 'This is not that kind of cancer which you understand. This is not a hard cancer like that which comes in the breast, and which is hopeless. You have a bad ulceration of the womb. It is not hopeless; there are cases which are cured.' In the case which I have mentioned, where the lady took thirty-five grains of morphia a day, the word 'cancer' never passed my lips, nor did it pass hers. None of the members of the family used that word, yet she knew as well as I did that it was a cancer. It was always spoken of as that 'bad ulceration.' About three years ago I learned a lesson on this point. I was asked by a physician to see a near relative of his. His suspicion was that it was a cancer. I said to him, 'Suppose that this is the case, shall I tell the lady?' He replied, 'Yes, she ought to know; tell her by all means.' After I had examined and found a carcinoma, I said, 'I am very sorry to say that this is malignant,' and then went on and told in so many words what the trouble was. She never rallied from that. She made up her mind that her days were numbered, and that there was no use in doing anything, and in a short time she died. I say, then, never tell a woman that she has a cancer.

"I have now made a funnel-shaped opening, into which I can readily introduce three or four fingers; before, I could barely get one in. I have not gotten into the bladder nor into the peritoneal cavity, but I am afraid if I go farther posteriorly, that I shall open Douglas' pouch. I can trace the cancerous tissue to the internal os, but it does not pass to the cavity of the womb.

"You see that while there has been some hæmorrhage, still it has not been alarming. Sometimes there is unpleasant hæmorrhage. During the operation you are not apt to have much hæmorrhage if you work rapidly, and quickly get down to healthy tissue. If hæmorrhage should occur, do not use Monsel's solution (the sub-sulphate of iron), for it makes plaster-like clots, and so corrugates and contracts the parts that you cannot continue the operation. Under these circumstances, ordinary cider vinegar serves an excellent purpose as a hæmostatic, without the inconveniences of Monsel's solution.

"Having removed as much as possible of this friable material, I purpose to apply fuming nitric acid to the raw surface. Usually, I prefer the application of Paquelin's thermo-cautery; but the instrument is out of order, and I do not think that I can reach all parts as well with the cautery as with a fluid. I apply the acid with a piece of cotton, allow it to remain for a short time, and remove by injecting water. I then again apply the acid. It is not necessary to use alkalies or oil to neutralise the acid. If enough water is injected, it will so dilute the acid that it cannot injure adjacent parts.

"There will be but little pain from the operation, but she will probably feel some soreness from the position in which the limbs have been held. When she is put to bed she will receive a suppository of the extract of opium (gr. j).

"I am sorry to say that these cases are very common. Cancer is, I think, on the increase; but why it is I cannot say. The disease more frequently affects the uterus than any other part of the body, unless it be the breast.

"You see that I have a little wound upon one of my fingers, but I am not afraid of inoculating myself with the cancerous matter, for I am in good health. If I were run down, it might be somewhat hazardous to get such offensive matter on a wound. It is the same with dissecting wounds, which occur usually towards the end of the session, and with those who are overworked. This is not the case with venereal disease. No matter whether the health is good or bad, one is liable to be inoculated with syphilis. Nothing would tempt me to thrust my finger into a vagina in which I knew there was a chancre. It was only yesterday that I was asked to take charge of a patient who had a chancre, but I absolutely refused to have anything to do with it. Some years ago I got caught. After examining a case, there appeared on my fingers a sore which would not heal. I showed it to Dr. Agnew, and he pronounced it to be a chancre. For awhile I believe that I was the most unhappy man in Philadelphia. The diagnosis, I think, was incorrect, for the sore disappeared, simply leaving a scar, and was never followed by any constitutional symptoms. A burnt child dreads the fire, and I cannot be hired to put my finger where I know there is a chancre. Winter before last, in one of the ward classes, after I and a number of the gentlemen had examined the uterus in one of our patients, she called attention to a sore in the vagina, which proved to be a chancre. Some of the gentlemen looked rather frightened, and I cannot say I liked it very well myself.

"A number of years ago, I attended a respectable woman in confinement. I then lost sight of her for several years, when she again wished me to attend her. When I called to see her, I noticed that she kept herself wrapped up. As on the previous occasion it had been necessary to use the forceps on account of the narrowness of the pelvis, I was prepared when the head would not come down, to apply the forceps. When I exposed her, I found the nates and buttocks one mass of venereal disease, and her neck was raw from the same trouble. I would have presented any gentleman with a hundred dollars to have applied the forceps and delivered the child. I stripped my arms to the elbows, and thoroughly applied a mixture of carbolic acid and vaseline. I then applied the forceps, using one hand only in the vagina. As soon as they were in position, I ran out of the room, and carefully washed my hands, and again applied the carbolised vaseline. I then delivered her, and again washed myself with the utmost care. For a number of days I waited anxiously to see what the result would be, but no bad effects followed."

ERRATUM.—Dr. J. Whitson desires us to correct an error which appeared in the heading of his paper in last month's *Journal*:—For Micro-photograph *read* Photo-micrograph.

THE

GLASGOW MEDICAL JOURNAL.

No. V. May, 1883.

ORIGINAL ARTICLES.

TISSUE REPAIR, OR THE PATHOLOGY OF THE SUBCUTANEOUS OPERATION BY INJECTION FOR THE CURE OF HERNIA.

By JOSEPH H. WARREN, A.M., M.D.,
Member of the British Medical Association; Member of the American Medical Association (Judiciary Council); Hon. Member of Vermont State Medical Society; Member of Massachusetts Medical Society.

Part II. Permanent Cure of Hernia by Subcutaneous Injection.

Tendinous Irritation.—It is strange that, in view of all the logical and scientific thinking of the present day, so many erroneous ideas should be perpetuated in regard to the permanent cure of hernia. Professor Gross, years ago, struck the keynote of the subject when he said that the only chance of curing this complaint lay in compression by a truss, or the inflammation produced by the subcutaneous injection of some fluid. Yet, how little is the pathology of such an operation understood, even at the present moment, by the great majority of medical men.

Dr. Heaton * ascribed his cures to a method of "*tendinous irritation*" without inflammation, attempting thereby to develop a pathology whose chief element is the total absence of all pathology; and on this account some operators have expected a miraculous cure to take place, but finding that the same laws govern this operation as govern all other surgical operations, have pretended to feel themselves lucky if they got any favourable result whatever from their labours. My object in this paper is to show that the operation has no peculiar pathology in it, that modern pathology corroborates at every step the teachings of clinical experience in the operation, and that those who still persist in assigning the cures by subcutaneous injection to some peculiar "tendinous irritation" without inflammation, are forgetful of their scientific training, and regardless of their reputation for professional consistency.

---

* Heaton *On Rupture*, edited by Bavenport.

This "tendinous irritation," so called, is an absolute myth; in reality we have a regenerative inflammation, the pathology of which I have already clearly described. Because the operation is so completely subcutaneous, this regenerative inflammation does not run on into the classic inflammation, as I have called it, nor develop granulation tissue. This is the only *rational method* of closing the hernial canal. On the other hand, all other operations for hernia fail in good results, because the intense inflammation which they inevitably tend to excite destroys their good intentions.

Wood's operation, about which, of late years, we have heard so much, has in it, indeed, *two* elements of failure. First, it seeks to draw together fibrous bands which are hard, firm, and immovable. This operation in itself excites theoretically very little local inflammation, and therefore there is no obvious reason that I can see why adhesion between these bands, so as to close the hernial canal, ever should take place. It is like operating for a lacerated cervix or hare-lip without paring the gaping edges. On this account, I suggested to Dr. Henry O. Marcy of Boston, U. S. A., the feasibility of freshening the fibrous edges of the hernial opening before occluding the canal by carbolised ligatures. This method, I understand, he has attempted, and I should think it offers a more rational chance of success than previous treatments by ligature.

The second objection to Wood's operation is, that when performed in the ordinary manner by wire sutures, it depends for its cure upon a process of granulation which, from its protracted duration, is fatal to a cure of hernia; and from its danger of producing an iliac abscess or a general pyæmia, may be fatal to the individual. When, however, operations which involve external incisions are performed under strictly antiseptic precautions, which reduce them theoretically to subcutaneous operations, the results are far more favourable than when the wounds are allowed to heal in the ordinary manner. This fact I have amply shown in my description of Czerny's method of operation in my *Practical Treatise on Hernia.*[*]

Closure of the sac.—The statement has been made that "the essence of success in hernial operations consists in setting up peritonitis in the sac, and in the ability to control the inflammation when started."[†] This, of course, can apply only to cases of congenital hernia, and even in these cases is true only in a very modified manner. The essential element of success in the cure of congenital hernia, as in the cure of acquired hernia, is the *closure of the hernial rings and hernial canal.* It is true that such closure will at the same time obliterate the unclosed pouch of peritoneum, but if this pouch be the only thing that is occluded, there is nothing to prevent another protrusion of the peritoneum through the canal, which is still unclosed, forming what is termed the "acquired congenital form of hernia," the "encysted hernia of Sir Astley Cooper," or the "infantile hernia of Hey."[‡] But even supposing that a limited degree of local peritonitis is the result of our treatment we do not fear the result. On the contrary, we are rather rejoiced that it should occur, since it tends to complete the cure by causing adhesion between the visceral layers of the sac, and also between the parietal layer and the fasciæ forming the hernial canal. The time has been when local peritonitis was the bugbear of abdominal surgery, but that has long ago passed away, as the many successful abdominal sections and ovariotomies bear witness. Certainly, as bold an operation as any subcutaneous injection was recently reported[§] from Dr. H. J. Bigelow's practice, where, for an umbilical hernia, the invaginated sac of the integument was blistered successively with tincture of cantharides, liquor ammoniæ fortior, and tincture of iodine, to produce intense suppuration in hope of a permanent cure. One hundred and sixteen days after entrance to the hospital, granulations were seen at the neck of the inverted sac; two hundred and two days after entrance the patient was allowed to sit up for a short time; and two hundred and fourteen days after entrance she was dismissed cured, but told to wear a truss as a matter of precaution. If the obliteration of such an organic plug as this invaginated hernial sac would not tend to produce local peritonitis, I fail to understand what would.

---

[*] *Treatise on Hernia.* Second edition. Boston: J. R. Osgood & Co. 1882.

[†] Dr. W. Cheever in *Boston Med. and Surg. Jour.* 12th Jan., 1882.

[‡] *Practical Treatise on Hernia,* p. 16.

[§] *Boston Med. and Surg. Jour.* 5th January, 1882.

It has been claimed that this method of procedure involved rather a new process of curing hernia. As long ago as the times of Ravin, Rivière, and Arnauld, *position* was recommended as a cure, while the process of inflaming a fold of invaginated skin is the very method which Gerdy and Signoroni practised in 1837, and which Würtzer modified.

Injection of the Sac.—A second error in regard to the subcutaneous method of injection is one that I have time and again emphatically contradicted. The profession will persist in supposing that the injection is to be made into the hernial sac, that is, into the peritoneal cavity, and consequently expect to see a general peritonitis, and perhaps an inflammation of the cord and testis, sympathetic or traumatic. This is the method described in most of the textbooks of surgery; but certainly a cure by such a process would be little short of a miracle. If this were the correct theory, we might as well expect that a sieve would hold water as that a peritoneum would support the pressure of the intestines, constantly forced down by gravity and the contraction of the abdominal muscles, into a perfectly open hernial canal. I say it once for all, the tissues to be irritated and organised into fibrous structure are the tendons and fasciæ forming and comprising both the internal and external hernial rings, together with the whole length of the hernial canal, and that the hernial sac is always to be thoroughly reduced before the operation whenever it is possible. If, perchance, the sac has become adherent to these rings or the canal, we can by a very little manipulation, such as most surgeons are capable of, lodge the point of the syringe between the sac and the borders of the canal, and deliver our injection *into* the fibrous tissue and *around* the hernial sac. As I have already said, a little inflammation of such a sac is not only not to be dreaded, but, on the contrary, rather to be desired.

Action of Quercus Alba.—A third error, and the last that I shall notice here, has reference to the astringent properties of the Quercus alba which is injected. None of the fluids employed by the early operators in subcutaneous injections for hernia were used with any idea that it was a fibrous *contraction* that was to be sought. On the contrary, the very first idea of this method was suggested to Velpeau by the injection method of curing hydrocele, which was by creating *inflammation* and *adhesion*. Never, before Heaton, had any one ever dreamed that it was to "the astringent principles of the injection," producing "a permanent contraction of the fibrous structures," that we must look for our regenerative process, and never again may we hear of such pathology. I am not surprised when I think of this, that surgeons have been sceptical of the benefits resulting from the injection method; but I am surprised when I hear medical men of the present day repeating and believing such false pathology.

The operation for the cure of hernia by subcutaneous injection is then successful, *not* because it produces a "tendinous irritation," that is, an immediate contraction of fibrous tissues, but *because it produces a local inflammation without suppuration.* This is an important distinction, both as regards the theory and as regards the practical success and permanence of the operation. All other methods have accepted and expected suppuration as an accompaniment of the operative procedure, and have not been disappointed when they obtained it. This method avoids suppuration as it would every other unfavourable complication. Other methods cannot avoid suppuration; this method can, and does avoid it, when properly performed, and with the proper fluids.*

As regards the comparative value of the fluids that have been used from time to time for injection, it is altogether probable that many have failed because they were absorbed without producing the amount of inflammation necessary to produce a reorganisation of fibrous tissue. Probably oak bark is specific in its action, because of all substances not destructive to the tissues, it refuses most obstinately to be absorbed before the inflammation set up by it, the ether and alcohol, shall have caused a sufficient cellular activity to render the regeneration of the normal fibrous tissue reasonably certain. All that by any possibility its astringent properties can do, is to aid perhaps in keeping the fibrous pillars of the hernial rings somewhat in their normal position, until the new tissue is properly organised into a firm growth.

Is this New Fibrous Tissue Stable?—Thus far we have considered the cure of hernia from the theoretical standpoint of the nature of the inflammatory process. What evidence can be adduced that the new fibrous tissue, which is formed by this regenerative inflammation, will be permanent and not readily absorbed? The proof that can be offered upon this question is very positive and twofold in its nature. First, the evidence drawn from analogy; second, the unmistakable evidence of clinical experience.

---

* With the fluid which I have latterly adopted and used in upwards of a hundred cases, I have not had a single case of suppuration. Among my first few cases, in which I used Heaton's mixture and instruments, I had three cases of suppuration—one severe and two slight cases.

Proof by Analogy.—By the evidence offered by analogy, I mean the history of the permanence of fibrous new growths and adhesions produced by inflammation in other organs and tissues of the body besides the hernial rings. In examining these fibrous tissues, I shall use for my chief reference the latest and most authoritative work on pathological histology, I refer to Cornil and Ranvier.

Let us first consider *arteries*[*] and *veins*.[†] Certainly every surgeon has abundant opportunity to see the inflammatory processes that occur in these tissues by the reorganisation which takes place in vessels that have been ligatured. The whole safety of the recovery depends upon the knowledge that the new tissue here formed will not become absorbed, but will endure as long as the arteries endure. Not only, however, is the growth of permanent tissue seen in traumatic inflammation, it is seen also in acute *endarteritis* and in *periarteritis*, occluding the vessels and hindering or even arresting the circulation of the blood. The same permanent new tissue is seen also in chronic *endocarditis*[‡] and in *valvulitis*,[§] in which latter process the chordæ tendiniæ of the auriculo-ventricular valves are also indurated and shortened. When, however, a suppuration occurs in or around these structures, the process is well known to be destructive and absorptive.

Inflammatory new growth of tissue, which is remarkably permanent and firm, is seen also in chronic *mastitis*,[||] chronic *adenitis*,[¶] chronic *bronchitis*,[**] *œsophagitis*[††] (where the resulting stenosis is often extreme), in cicatrisation of *ulcers of the stomach*,[‡‡] producing the hour glass constriction, and in cicatrisation of *intestinal ulcers*, whether tubercular, syphilitic, typhoid, or dysenteric. The result of such fibrous cicatrisation is "obstinate constipation, and even complete obstruction of bowels."[§§] In *cirrhosis of the liver*,[||||] and in *fibroid phthisis*, so-called,[¶¶] the same inflammatory fibrous growth, from its remarkable persistence, calls forth the gravest prognosis. In the fibrous form of *interstitial nephritis*,[***] the characteristic and *incurable* lesion is the formation of new connective tissue, embryonic or fibrous, and atrophy of the secreting glands. Not infrequently as the result of a simple *orchitis*[†††] do indurations, consisting of hard and contracting cicatricial tissue, compress the excretory ducts of the testicle, causing a narrowing or entire obliteration of them, and consequently a suppression of the functions and an atrophy of the testicle.

---

[*] P. 307.
[†] P. 339.
[‡] P. 300.
[§] P. 303.
[||] P. 707.
[¶] P. 353.
[**] P. 405.
[††] P. 459.
[‡‡] P. 471.
[§§] J. J. Woodward in *Med. and Surg. Hist. of War of Rebellion.* Med. vol., Part ii, Vol. ii, p. 503.
[||||] Flint, *Principles and Practice of Med.* 3rd ed., p. 607.
[¶¶] *Ibid.* P. 223.
[***] Cornil and Ranvier, p. 631.
[†††] *Ibid.* P. 660.

The time has been when *inflammation of the urethra* producing stricture was considered a special type, but now I think it is agreed that the type is common to all fibrous tissues, since stricture may occur not only as the result of gonorrhœa, but also before puberty, and as the result of a traumatic injury. The locality of a stricture is important in relation to its pathology. By far the majority of cases occur at the junction of the spongy and membranous portions of the urethra, where the surrounding connective tissue is loose and comparatively superficial. As, however, we approach the prostatic end of the membranous portion, where the connective tissue is dense, and firm, and deeply seated, stricture becomes rare in occurrence. The rule seems to be that stricture occurs most readily where the opportunity for the deposit and organisation of inflammatory material is the greatest. This is precisely the pathology we have presented in regard to the permanent cure of hernia by subcutaneous injection.

The results of *inflammation of serous membranes* * are well known to every physician who has ever made or seen an autopsy. Few indeed are the subjects who do not present adhesions of the pleuræ, either band-like or even obliterating the pleural cavity. Sometimes these bands are seen in the pericardial sac,† while after the various forms of abdominal inflammation cases are by no means rare where the whole length of the intestines, with all the abdominal organs, has been fused into one solid mass by firm and persistent adhesions. More common indeed are the firm local adhesions resulting from chronic *ovaritis*, *salpingitis*, and *perimetritis*.

In *arthritis*,‡ whether acute or chronic, traumatic or rheumatic, the synovial membrane is not the only part affected, but even in slight attacks a modification of the diarthroidal cartilage is constantly seen. The histological process is very readily appreciated by the microscope, while many a sufferer can testify that the thickening and stiffening of rheumatic joints is one of the most obstinate of tissue lesions to overcome. On the other hand, in those forms of arthritis which go on to an intense inflammation and even suppuration, what lesions do we find? An erosion and ulceration of tissues, a caries of bone, and finally a complete destruction and absorption of all surrounding parts.

---

* P. 259.

† P. 290.

‡ P. 228.

In myositis, as seen after wounds, the new formation is firm and fibrous in its nature, forming, as Billroth expresses it, an "inscriptio tendinea." In his opinion, the process never goes beyond the formation of tendinous tissue, but Weber and Gussenbauer maintain that a new growth of muscular tissue gradually develops. At any rate, the healing process is complete unless the inflammation has been so severe as to be suppurative. In such cases the muscular fasciculi undergo fatty degeneration and ultimately die. In this connection I would mention the comparatively rare condition known as fibroid induration of the heart.* This, in most cases is a result of myocarditis, and is induced by inflammation of the peri- or endocardium, or in some cases by syphilis. It consists of the development of a fibrillated tissue between the muscular fasciculi, which is so progressive in its growth that it causes atrophy and fatty degeneration of the muscles, and is one of the gravest of cardiac diseases.

Finally, let us consider inflammations of the skin. No lesion which does not destroy the papillary layer produces a scar or cicatrix.† When the papillary layer has been destroyed by ulceration, deep burns, scalds or wounds, a firm contracting cicatrix is formed, "permanent, continuing to exist through life with but little alteration."‡ The fearful contortions produced by the contraction of scars from burns and scalds are an every day occurrence, and require the utmost skill of the patient surgeon to overcome or even relieve. The same contortions are seen in injuries of deeper parts, as muscles, fasciæ, and tendons. It is true these are all formed by a process of suppuration, but the contraction is well marked, *because* there is no strain put upon the tissues to force them apart continually as there is in the curing of a hernia. Indeed, treatment by mechanical extension is one of the best methods of overcoming the deformity caused by a burn.§ On the other hand, it is a well known fact that scars tend very gradually to become somewhat softened and obliterated; and, in accordance with the law I have deduced, that repair processes tend to reproduce the tissue of the region where they are seated; that is, tend as regards the skin to develop gradually rudimentary papillæ and epidermis.

Since regenerated fibrous tissue tends strongly to contract, why is it that the operation of tenotomy relieves so much distortion and produces such good results? Simply because the tendinous cicatrix, *before* it has fully organised, is more readily stretched than the original tendon of the muscle. "Tenotomy proves useful only from the orthopædic after-treatment." When, therefore, suppuration unfortunately intervenes, the apparatus cannot be applied until the cicatrix is more firmly organised, and, as a consequence, the benefit of the operation is far less. The same principle is true in the operation for strabismus. The muscle opposite to the one that has been divided rotates the eye so that the severed muscle shall gain a new attachment farther back. Therefore, "it is better that the eyes should be moved about while reunion of the muscle is taking place, and especially that they should be turned in the opposite direction from that of the former squint,"* otherwise their condition may be as bad as before the operation.

---

* Green's *Pathology*, third Amer. ed., p. 230.

† Cornil and Ranvier, p. 734.

‡ Duhring, *Diseases of the Skin*, p. 54.

§ Byrant, Billroth.

My objects in thus discussing the various connective tissues of the body have been these:—

1. To demonstrate that all fibrous tissues of the body tend, when inflamed, not only to contraction, but to a permanence of contraction.

2. That it requires the greatest patience, together with considerable mechanical skill, to overcome such permanent contractions when they have become fully organised.

3. That while the inflammatory products are in the course of organisation and have not developed into firm and stable normal tissue, they may, by the intervention of other processes tend either to resolution or to suppuration, both events resulting in destruction and absorption of inflammatory new growth either from a slow and imperfect healing, or from the antagonism of natural mechanical forces at work.

4. That in no case have we seen any evidence of such resolution or absorption of inflammatory new growth when once it has become fully organised; therefore we must reason illogically when we make hernia an exception to the general rule.

We have, then, seen the truth of the general law deduced by Billroth, and by Cornil and Ranvier, that all processes of repair tend to reproduce the tissue of the region where they are situated; that is, that these processes of repair are progressive in their nature until they attain a certain structure, which structure then becomes stable, as stable as normal tissue, and subject to the same laws of growth and decay. If a person has naturally weak abdominal fasciæ, how can any one expect a strengthening of one local portion to strengthen the whole abdominal area? In other words, it would be no unexpected event for a person to be permanently cured of an oblique inguinal hernia and then to suffer afterward from a direct hernia. Is this an argument against the value of the method of subcutaneous injection? Yet many talk as if they thought it was. In other words, the return of a hernia once retained, is not the only evidence that is essential. We must know *first* whether the fibrous tissue has ever seemed to be firmly organised, and *secondly*, whether the new hernia is precisely in the same locality as the original lesion. I have seen several cases and am cognisant of others where it was not.

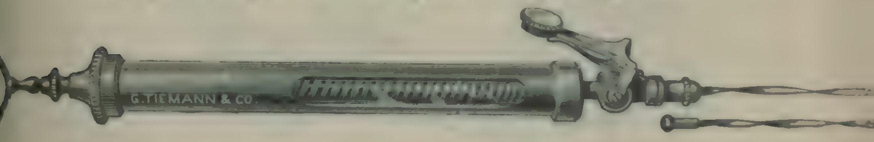
---

* Williams, *Diseases of the Eye*, p. 327.

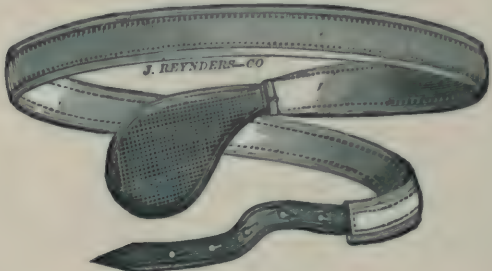
Clinical Experience.—We have lastly to consider the results of clinical experience. It is perfectly in accordance with the wise conservatism of medical and surgical science that, before an operation shall become accepted and be admitted into general practice, it shall be able to produce as the result of a long and careful experience, such a percentage of complete recoveries as to render its adoption and approval by the profession an advance in the progress of the healing art. The objection has, however, been made to subcutaneous injection for the cure of hernia, that only a few trustworthy cures have been brought to the notice of medical men. That there might have been a valid objection to the operation before I took it up I will not deny, but certainly there has been some stimulus at work of recent years that has brought to the notice of the profession many trustworthy cases of the permanent cure of hernia by this method. It is now a number of years since I first operated for hernia by subcutaneous injection, and I well remember that when, about four years ago, I read a paper upon the subject before the Suffolk District Medical Society, a worthy member thought two years of relief from the return of hernia would insure the permanence of the cure. Fully that time has now gone by and I have still been operating, and with but few exceptions I have not seen a case which, once pronounced cured, has had a return of hernia at the precise locality where I had operated. Nor do I stand alone in making such a statement. Many of my cases have been seen and thoroughly examined by my professional brethren, both in this city and out of it, whilst the reports of other surgeons in this country, in regard to their success, are as emphatic and conclusive as my own. We certainly, then, are not at fault if successful cures are not yet known to the profession. Not only, however, have I met with success in arousing the profession to accept the operation as one of permanent value, but I have succeeded in proving it a safe procedure. Some of our best surgeons who once considered it rather hazardous, have now emphatically said that the operation is free from danger. Nor is this all. There are few other operations that have presented, or that can present, such a list of favourable results in the practice of trustworthy physicians. It is not necessary to quote cases taken from my own private practice. I trust it will be sufficient for me to say that my own clinical experience as well as that of other medical gentlemen fully support whatever may have been said in favour of this operation.

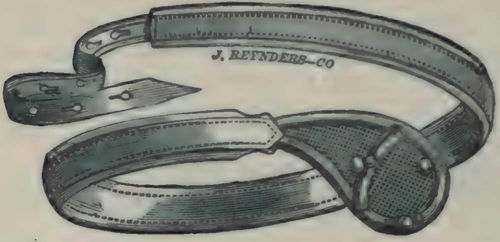
Conclusion.—I have found, however, that there is an urgent necessity in the majority of cases for a more stimulating injecting fluid than the simple extract of oak bark, and for an instrument which will more thoroughly and more effectually distribute the fluid upon the hernial rings and canal. Nevertheless, I should think that for children of the age of four or five, the simple extract, or the formula which in my treatise I have called formula A, is amply sufficient. Upon infants and children under four years of age I still refuse to operate, because I consider the pressure of a good bandage or truss is better. There are surgeons, however, who operate upon infants in arms, and they report good results.

I attribute my success in the operation mainly to three things. (1st.) The use of a stimulating mixture, (Formula C.) (2nd.) The use of an instrument the needle of which revolves and distributes the fluid more effectually than any instrument made upon the pattern of a hypodermic syringe:—



(This instrument is finely made by George Tiemann of New York.) (3rd.) The use of a truss as soon as possible after the operation. It is evident that an ordinary truss cannot be very early applied to the parts which are in a state of inflammation. For this reason I devised a truss, the pad of which is flat, and made of silver wire gauze. I call it the anatomical truss. It tends to keep the rings together, rather than to force them apart, as every stuffed pad must do. External applications can be made to the parts beneath if they become irritated or inflamed, and it is light and capable of such accurate adaptation to the tissues that it can be worn constantly with perfect comfort to the patient. I apply it eight or ten days after I have injected the hernial rings, and the patient wears it night and day constantly for two or three months. For illustration I will cite three cases, one of direct hernia, operated on in June 1881, and the truss applied on the eighth day; another, double indirect, operated on in October 1881, and truss likewise applied; and a third case of oblique inguinal, operated on about a year ago, the truss being applied ten days after. I have seen these cases several times since the operation, and know that they remain cured and they do not wear trusses. The truss is equally valuable for a patient who has had no operation performed, and will be found to give more relief than any padded truss.





In view of the interest which has been shown in the method for the cure of hernia by injection, and owing to the fact that but few are familiar with the method and its application, I have prepared the following brief outline of the method, thinking that it might be of value to the readers of this paper.

This operation is no longer an experimental one nor of doubtful expediency, but has been fully established as safe and expedient. Much of the success of the operation depends upon the judgment of the operator, as shown in the selection of cases and in the after treatment. The injection will cure in cases irrespective of age; but the most favourable age is from four to twenty-one years. Before the age of four I do not think it expedient to operate. After twenty-one more care is required to obtain good results; but the best results are often obtained in those of advanced age. Sex seems to have no influence for good or bad. Occupation may have an after result in reproducing hernia at the same place as the first, if too great a strain is brought upon the place too soon after the operation, or near it where the first hernia is retained, a direct rupture occurring through weakened abdominal walls.

As in all operations, the success is greatest, other things being equal, in those of a healthy state. Constitutional disease, however caused, retards the process of cure and may prevent it. In such cases, tonics (as quinine or iron), to counteract the general debility, and specific remedies (mercury, iodides, &c.), to counteract the specific diseases are indicated, the operation being delayed until the system is built up.

Nervousness is to be considered on account of the time that is to be spent in bed on the back, and the nervous system, if highly excitable, should be toned down by appropriate remedies. An irritable spine is unfavourable to the operation.

The kinds of hernia enumerated in the order of the best results and least danger are inguinal, umbilical, and femoral. Large congenital herniæ in persons over 40 are very unsatisfactory, as the two rings are practically fused into one, and there is no canal left for the injection to act on.

The hernia must be reduced. Irreducible hernia may often be reduced by patient trials. Taxis is to be aided by position, by ether, by ice poultices, and by subcutaneous injections of morphia sulphate and atropia sulphate, combined or separately. Adhesions may be broken by manipulation or by subcutaneous division, or the tumour may be gradually reduced by an elastic cup truss.

The hernia being reduced, the middle finger of the left hand is carried up into the opening of the external ring, the scrotum being invaginated. The reduced intestine is held back by this finger. The tip of the index finger placed in its natural position by the side of the middle finger, points to the site of the puncture for the needle of the syringe. This puncture is opposite the external ring. It is not made through the scrotum, but through the skin of the abdomen in the inguinal region. The needle is passed carefully into the canal, avoiding the cord and the vessels. It is evident that the operator must be a good anatomist and a skilled surgeon. The finger being in position the fluid is forcibly injected along the tract of the canal—not into the sac nor into the peritoneal cavity. The fluid is still injected as the needle is withdrawn, and at the same time the needle is revolved so that every part of the canal shall be bathed with fluid. The syringe which I have devised for my own use does this automatically, for, on opening the valve by pressing the lever, the spring on the piston rod forces out the fluid through the holes in the sides of the needle, which is at the same time rotated by the act of withdrawal, owing to its twisted form. I do not say that this instrument is necessary for a perfect result, but it makes the operation easier and simplifies the injection, so that the result is more likely to be favourable. The common hypodermic syringe will not answer, for it delivers the fluid against the sac, and not on the walls of the canal; and the scarf point is dangerous, as arteries may be wounded by it. An instrument which delivers the fluid only in drops is not of as much value as one that sprays the fluid forcibly on to the tissues.

When the injection has been made the ring will be felt to contract like a sphincter round the tip of the finger, and from my experience I do not expect a favourable result unless I feel this contraction. When the needle is withdrawn the finger is to be placed over the puncture to prevent the escape of fluid.

This is also guarded against by making the puncture with the skin drawn tense and thus making a valve-like puncture.

The amount of fluid varies with the size of the hernia and the irritability of the tissues. The larger the hernia and the older the patient, the more stimulating should be the fluid, and the greater the amount injected. Inguinal hernia requires the greatest amount, while femoral requires the least—about one-half as much as inguinal.

The fluid that I use is made according to one of the three following formulæ:—

Formula A.—For infants and children, whether the hernia be accidental or congenital.

℞	Fl. ext. Quercus albæ, ...	℥ij
	Reduced by distillation to . .	℥j
	Alcohol (90 per cent), ...	ʒij
	Ether sulph., ...	ʒj
	Morph. sulph., ...	gr. ss. M.

Sig. Inject 8-10 minims.

Formula B.—For old and long standing herniæ, whether congenital or acquired.

℞	Fl. ext. Quercus albæ,	℥iv	
	Reduced by distillation to	℥j	
	Alcohol (90 per cent),	ʒiij	
	Ether sulph.,	ʒij	
	Morph. sulph.,	gr. ij	M.

Sig. Inject 10-25 minims.

Formula C.—Best in the majority of cases.

℞	Fl. ext. Quercus albæ,	℥vj	
	Reduced by distillation to	℥ii	
	Alcohol (90 per cent),	℥ss	
	Ether sulph.,	ʒii	
	Morph. sulph..	gr. iv	
	Tinc. veratri viridis,	ʒii	M.

Sig. Inject 15-20 minims in small and recent herniæ; but 25-50 minims in large or old herniæ.

Note.—This fluid will cause a marked reduction of pulse and temperature, and it may be necessary to put a hot water bottle to the patient's feet. This reduction may last as long as 48 hours, and gives a decided advantage in obtaining a more decided local effect of the irritant.

By this combination I think that the best results will be obtained. Other remedies have been used, as iodine, ether, alcohol, sulphate of zinc, &c. The object of any fluid that is used is to cause irritation and set up effusion. Carbolic acid and similar fluids will not effect this, and consequently are not of use.

The injection is performed with the patient lying in bed, on his back, and being completed, a compress and spica bandage are applied. Pain and restlessness are to be subdued by sulphate of morphia, and a compress of cold water may be placed over the site of the operation if the pain is intense. Quiet and maintenance of the position on the back are to be insisted upon; but if the patient, for some urgent reason, turns upon his side or is restless within the twenty-four hours following the operation, the success of the operation is not of necessity destroyed, although it is endangered. The bowels are to be moved just before the operation and not again for several days. The urine may have to be drawn, but this is the exception. Light food is to be given and quiet maintained.

The slight fever that follows does not need medication as a rule. The injection is to be aided by rest and position, and by the pressure of a spica bandage. In eight or ten days, if everything has gone well, the patient may stand up, the operator supporting the parts with the hand, and if the opening is felt to be occluded, we may fit a light truss and let the patient go, cautioning him to favour himself for a short time. In a few months another examination is to be made, and if all is well the truss may be dispensed with.

If, during the treatment, an abscess forms, it is to be treated according to rule, being opened antiseptically, and constitutional symptoms met as they arise. An abscess should not occur, and shows that there has been an error in judgment, either in selecting the case, or in using too much or too stimulating a fluid. Every swelling is not an abscess. A swelling simulating an abscess may appear a few days after the operation, but in reality this is only effused lymph. This will be discoloured by the injection, and look as if there was decomposing blood in it. A few days will decide whether this is an abscess or not, as it will be absorbed or "point."

As a final result we may confidently expect a cure. In extreme cases more than one injection may be necessary. In no case is the operation dangerous any more than hypodermic medication is dangerous; and if it is not fully successful, it will at least better the state of the patient, inasmuch as it will contract the rings in a degree, and tend to retain the hernia by as much as the rings are contracted.

The object of the operation is to set up inflammation of a mild type in the fibrous tissues of the canal and rings. It does not aim to set up inflammation in the sac, nor to cause peritonitis, nor to set up an imaginative tendinous irritation. Neither does it avoid inflammation altogether.

THE TUBERCULAR BACILLUS AND ITS RELATION TO PHTHISIS PULMONALIS.

By JOHN LINDSAY STEVEN, M.B.,
Pathological Chemist to the Western Infirmary, and Assistant to the Professor of Clinical Medicine in Glasgow University.

(*Being a Paper read to the Glasgow University Medico-Chirurgical Society, 9th February, 1883.*)

Mr. President and Gentlemen,—In the whole range of modern medicine there has perhaps been no more vexed question than that of the relationship existing between tuberculosis and phthisis pulmonalis. The almost invariable association of tubercles in some form or other with all true cases of phthisis, has made it abundantly evident that a relationship, and that a very close one, does subsist between them. The great difficulty, however (and even yet it may well be doubted whether a true solution of the problem has been arrived at), has always been to explain its true nature. Again, the very close comparison that may be drawn between the essential features of phthisis and those observed in other constitutional diseases known to depend on the presence of a virus, has for a lengthened period given rise to the idea that it too may be dependent upon the presence of a specific poison; and this view has been obtaining a stronger and stronger hold upon the professional mind ever since Villemin demonstrated the possibility of communicating tubercular disease by inoculation. While, therefore, the idea of a virus in tubercle cannot be regarded as in any sense a new one, no one can deny that an entirely new aspect has been given to the whole question since Koch, in the spring of last year, made the very important announcement of his discovery of a bacillus in tuberculosis. And although Cohnheim, along with others, in confirmation of Villemin's observations, has given additional very strong proof of the existence of a virus in tubercle, yet even he, in the newest edition of his lectures, as Koch in his paper points out,* is forced to admit that, as yet, the actual demonstration of the tubercular virus has not been made.

Having thus generally indicated the nature of the question involved, it is my object, first, to point out how the bacillus of tubercle may be demonstrated; next, to state what is known of its life history, and habitat; and lastly, to see whether it throws any new light upon the relationship of tubercle and phthisis, and also how previous views must be modified in presence of this discovery.

Koch points out that, after trying all the known methods for the detection of bacilli, and failing to demonstrate the presence of any in tubercles, he was obliged to seek out some other mode, of which the following is a general account. He made a staining solution of methyl blue in this way, viz.:—200 ccms. of distilled water were mixed with 1 ccm. of a concentrated alcoholic solution of methyl blue, and to this, with repeated shaking, was added ·2 ccm. of a 10 per cent solution of liquor potassæ. After remaining in this fluid for twenty-four hours, the specimens were next immersed in a concentrated solution of vesuvin (Bismarck brown). When so treated the organisms appeared as small blue rods in a brown ground.*

---

* "Die Ætiologie der Tuberculose," von Dr. Robert Koch. *Berl. Klin. Wochenschr.* No. 15, 1882.

Although by this method the bacilli could be demonstrated, yet the colouring was not satisfactory, and soon afterwards Ehrlich devised a new staining solution, the formula and manner of using which will be found in Dr. Heron's paper in the *British Medical Journal*, 14th October, 1882, p. 735. In the same journal will be found an account of a magenta solution, by Dr. Heneage Gibbes, who seems to have obtained very good results with it. In Leipzig, Professor Weigert and Dr. Huber, of the Pathological Institute, use different strengths of gentian-violet solution, prepared after Ehrlich's method. The dyes of which I have practical experience in searching for the bacillus are those of Ehrlich and Huber. Dr. Huber's formula, as given to me by himself, is this:—"Gentian-violet (B. R. 26), 1 gram., dissolved in 30 parts of the following fluid: anilin oil and distilled water shaken together, and filtered; of this fluid 30 gram. to Gentian-violet 1 gram." The sections, after remaining in this fluid for 12 hours, are treated with a 33½ per cent solution of nitric acid, then stained in Bismarck brown, and mounted in Canada balsam. The results obtained by this method are very good, but it seems to me that the violet colour is apt to fade after the specimens are kept for a time. In my later observations I have used almost entirely Ehrlich's method, the formula for which is to be found in Dr. Heron's paper noted above, the particular dye used being Rosein No. 2, supplied by Messrs. Craig & Rose of this city. In my hands, with regard to the examination of the sputum, and in Dr. Coats,' as regards the lung tissue, this staining reagent has given excellent results, and I do not think that any one could desire a better demonstration of microscopic organisms than is to be got in this way.

I have now briefly to describe to you the details of the examination, and first as regards the sputum. A small fragment of the expectoration is laid on a fine coverglass, and spread out into a very thin layer, the best mode of accomplishing this successfully being to squeeze it between two coverglasses. The thin layer is then dried in the air, and passed rapidly once or twice through the flame of a spirit lamp or Bunsen burner. With the dried layer of sputum undermost, the coverglass is next floated on to the surface of the rosein solution, and kept there for half an hour, although, if convenient, no harm results from leaving it all night. It is then passed through a solution of nitric acid (1-2) till the colour is apparently all gone, and washed in water. After washing, the colour reappears to some extent, the proper tint being a pinky white. The coverglass is then placed in a strong, watery solution of methyl-blue or green for ten minutes or longer, washed, and examined in a drop of water. If bacilli are found, it may be mounted permanently in Canada balsam. The only difference in examining the tissues is that the sections must be kept in the staining fluid for at least twelve hours, after which they are treated in the same way, washed in alcohol, passed through oil of cloves, and mounted in Canada balsam.

---

* *Loc. cit.*

If the bacilli are present, they will be recognised as minute rods, nearly the length of a red blood corpuscle, very thin, and stained of a bright red colour, which strikingly contrasts with the blue ground in which they lie. The use of Abbe's condenser greatly aids in their detection, and, with the help of this illuminating apparatus, they may be easily seen with a lens magnifying 350 diameters, as you may see for yourselves under two of the microscopes on the table. In many cases the rods are seen to be distinctly jointed, as if containing spores in their interior. According to the observations of Koch, the organisms are of very slow growth, and they may, with the precautions which he describes, be cultivated outside of the body. They seem to be very tenacious of life—so much so, that the drying of a sputum, in which they are contained, does not impair their vitality, a very important feature from a sanitary point of view.

With regard to the habitat of the organism, Koch has, I think, very conclusively shown that it is to be found in every organ affected with true tubercular disease. He has found it in such diseases both in man and in the lower animals. The special site of the organism seems to be at the margin of caseous masses, at the edges of tubercles whose centres have become caseous, and in the centre of those containing recent giant cells. Koch's figures, given in the paper already referred to, are these:—He found bacilli in 11 cases of miliary tuberculosis; 12 of caseous bronchitis and pneumonia (6 with cavity); 1 of solitary tubercular tumour of the brain; 2 of tubercular ulcer of the bowel; 3 of freshly extirpated joints; and 4 of fungous inflammation of the joints. Somewhat similar figures are given as to the results in the lower animals, which I need not here detail. These results have been confirmed by further observers, and I may mention the work of Dr. Heron, who has found the bacillus in the sputum of 62 tubercular patients.

I have succeeded myself in detecting the organism in a considerable number of phthisical sputa, and also in the following tissues:—In the kidney, the seat of miliary tubercles; in the Fallopian tube, the seat of tubercular disease; in the broken down tissue of the lung in phthisis; and in the liver of a fowl which died of spontaneous tuberculosis. In the case of the latter, which I examined in the Pathological Institute of Leipzig, miliary tubercles were found scattered through the hepatic tissue, and the organisms occurred mostly, in groups or individually, at the margins of the tubercles.

Having thus, as clearly and as briefly as I can, pointed out to you the method of detecting, and the characters and habitat of, the organisms, the next question is to consider if this discovery throws any additional light upon our knowledge of tubercle. First of all, then, it places beyond possibility of doubt, what all pathological research has been pointing to for a long time, that tubercle is a disease dependent upon the presence of a specific virus in the shape of an organism. It proves, I think, that the miliary tubercle depends for its very existence upon the presence of this organism, and that it is originated simply and solely by the irritation which it or its products set up. And not only the tubercle, but those low forms of inflammation described as tubercular (caseous pneumonia, for example) may be regarded as excited in the same way. There is reason, too, I think, for believing that the irritation so set up is not to be regarded as in all respects similar to that set up in simple non-specific inflammation, either in its nature or results. No one can deny the very close comparison that may be drawn between a tubercle and a simple inflammatory product; but there are, to my mind at least, these striking points of difference:—(1) The abundant giant cells in tubercle as compared with their paucity in inflammation; (2) the comparatively well demarcated and defined area of the tubercle as compared with the shading off of a simple inflammation into the surrounding tissues; (3) the absence of vessels in the tubercle as compared with the vascularity of inflammation; and (4) the almost invariable tendency to caseation in tubercles, fibrous change, if it does occur at all, being very imperfect. Now, it seems to me that these differences may possibly be explained by the mode in which the bacillus may be supposed to act in producing the tubercle. As Koch has shown, we have here an irritant of excessively slow growth, and one requiring to be long in contact with the tissues before manifesting itself to any considerable extent. Such an irritant may well be supposed to act locally in the strictest sense of the term—*i.e.*, only in its own immediate neighbourhood, and to be incapable of setting up the reflex and paralysing effects necessary to involve the blood-vessels to the extent with which we are so well acquainted in cases of simple inflammation. The result will be that only the nearest cells—endothelial, epithelial, connective tissue corpuscles, &c.—will be mainly acted upon; no transudation from blood-vessels will take place, and no new blood-vessels will form. The affected cells, however, from the effects of the irritant, will proliferate in an unhealthy manner, and, partly from the absence of vessels, partly from the necrosing tendency of the organism, will soon caseate. The bacillus, keeping up the same process at the margins of the mass, will give to it its infective characters. It will thus be seen that neither tubercle or tubercular inflammation is to be regarded as strictly analogous to, if I may call it so, normal inflammation, and that this deviation from the normal course is to be attributed to the action of the bacillus. I may here remark that such a view is in accordance with that expressed by Cohnheim, and ably defended by Dr. Coats at a meeting of the Pathological and Clinical Society two years ago, Dr. Coats stating that he regarded both the miliary tubercle itself and the inflammation with which it is so frequently associated as dependent upon the then unknown, but firmly believed in, tubercular virus.

So much then, gentlemen, for the tubercle bacillus. We have now to inquire, Does this discovery modify in any way our views of phthisis pulmonalis? And here we enter upon a subject of the very greatest difficulty, and one which I approach with the utmost diffidence. I shall confine myself almost wholly, though perhaps not entirely, to the pathological aspect of the question as being that with which I am most acquainted. Before going further, too, I must also say that in what follows I shall frequently refer to the discussion held on this subject two years ago in the Glasgow Pathological and Clinical Society, and reported in full in the *Glasgow Medical Journal* for April, 1881.

First of all, then, we must clearly define what we understand by the term phthisis pulmonalis. It would be obviously wrong to make use of the term as applying to a condition purely local and special, as Dr. Hamilton has done in the discussion mentioned above; but so, too, would it, I think, be erroneous to object to Hamilton's use of the term, as Dr. Finlayson did, simply because by using it in a more general sense we can put a name on a disease at the bedside which *post-mortem* may turn out to be one of many conditions difficult to differentiate during life. Dr. Finlayson said, "his (Dr. Hamilton's) definition of phthisis pulmonalis seemed even more unsatisfactory, excluding, as he stated it did, something like one half of the cases regarded as phthisis by hospital physicians during life, and probably also so regarded after the inspection by many of them at least. . . . He must, however, protest, as a physician, against the definition given of phthisis pulmonalis, not merely on the etymological and historical grounds urged by Dr. Gairdner, but even on the narrower ground of its forming a distinction between local lesions which were thus unwarrantably separated from each other." I prefer to take as a definition what Dr. Gairdner says—viz., that phthisis pulmonalis is a "wasting of the whole body, accompanied by predominating pulmonary symptoms, which the pathologist now well knows to be in general significant of what used to be called tubercular disease in the lungs and elsewhere;" and to indicate my opinion that most true cases of phthisis will be found to be tubercular. Therefore, pathologically, though not perhaps clinically, that only can be regarded as true phthisis pulmonalis which is tubercular, thus coming back very closely to the view of Læннec.

Leaving the question of definitions, we have now to ask, Does Koch's discovery throw any light on the true relationship existing between tubercle and phthisis? I think it does. It lays to rest the question that tubercular disease of the lungs is secondary, in the sense employed by Niemeyer and Buhl. By many, and especially by Cohnheim, this view was set aside long before the time of Koch's discovery. If the views I have previously expressed as to the local action of the bacillus be in any degree correct, then the virus is to be looked upon as the first cause of phthisis, producing both tubercle and caseating pneumonia according to circumstances. Catarrhal pneumonia would thus be as truly a tubercular condition as the formation of a miliary tubercle is, and thus both are essentially similar as regards their etiology. We have no need now to search for a caseous focus to explain the occurrence of tubercles in the lungs, or to urge the necessity of a preceding catarrhal pneumonia, which, in the absence of mitral disease or Bright's disease to keep it moist, dries in, and thus forms the irritant setting up tubercle. We have no need now to fear that patients suffering from caseous pneumonia may become tubercular, because unfortunately there seems but too good reason for supposing that they are in the vast majority of cases tubercular already. In fact, Cohnheim, in his pamphlet,* departed altogether from histological conditions in the differentiation of tubercle, and said the only true test of tubercle was whether it would produce tubercle in another animal. The great difference between the view of Cohnheim and that of Niemeyer and Hamilton seems to be this. Hamilton believed that any caseous matter, however produced, would cause tuberculosis, Cohnheim said no—only caseous matter which is tubercular in origin, and which, therefore, contains the tubercular virus is capable of reproducing tubercle. Koch's discovery seems to place the correctness of the latter opinion on a very secure footing. It will be seen, too, that what we have stated above is a return in some degree to the view held by Lænnec; but it is not entirely so. The great point with which Lænnec seems to have been impressed was that the occurrence of the miliary tubercle was primary to all the secondary changes in the lung. But that this would not explain all is apparent by his having to describe a tuberculous infiltration—what we would now perhaps call a catarrhal pneumonia. Now, with our present knowledge, there is no such need for regarding the miliary tubercle as really the primary factor, for, as we have seen, the bacillus may be the starting point of both miliary tubercle and caseous pneumonia.

So far we have considered the relationship of the virus to caseous phthisis, we have now to consider the second great variety of phthisis—viz., fibroid phthisis. What light does the discovery throw upon this condition? Here, at the outset, it will be well to dismiss from consideration simple interstitial pneumonia, such as is obviously caused by the inhalation of irritating particles, as it manifestly can have nothing whatever to do—primarily at least—with tuberculosis. In addition to these cases, however, I find, in the discussion before referred to, that Dr. Coats describes cases of cirrhosis of the lung with large cavities, no caseation, and tubercles. Now, my pathological experience is by no means so great as to allow me for one moment to question the facts here stated—nor have I the least desire to do so—but, after the views I have already expressed, you will see that I have great difficulty in accepting the opinion that here the interstitial new formation and the tubercles are both the result of the tubercular virus. The tubercles, if they are present, must be due to it. But my main reason for not at once conceding that the inflammatory new formation in this case is also due to the virus is this—viz., the very great divergence which the new formation here presents from what we know to be the usual course of a truly tubercular process. We have already seen that the greatest feature of the tubercular virus is its tendency to set up caseous necrosis wherever it occurs. And here we are asked to believe that it possesses the altogether opposite power of causing the formation of new tissue. The only way definitely to settle this point will be to make a careful examination of such cases and see if we can find bacilli both in tubercles and inflammatory products alike, as we do in the caseous form. Again, this form of phthisis is rare as compared with the caseous form, and it seems to me a more natural theory to suppose that here we have an interstitial pneumonia complicated by tubercle, to the ravages of which the diseased lung is rendered liable, than to affirm that both phenomena in this case are due to the virus. Further, I don't think it right to consider as cases of true fibroid phthisis those where we have well marked and extensive caseation as well. The chronic low caseous formation going on in the lung, may well, apart from the tubercular virus which, as we have seen, acts in a special way, set up a certain amount of interstitial change, just as the gumma, due to a virus strictly localised in a particular spot, may do in a case of late syphilis.

---

* *Die Tuberkulose vom Standpunkte der Infectionslehre.* Von Julius Cohnheim. Leipzig: A. Edelmann. 1881.

The foregoing class of cases, includes, I think, the great majority of cases of phthisis pulmonalis, but in order not to limit the term too closely to one particular morbid process, it would be well to employ the term tubercular phthisis. We should also, as Cohnheim and Koch suggest, make the crucial diagnostic distinction, apart from all anatomical or histological detail whatsoever, the detection of the tubercular bacillus. Clinically, an interstitial pneumonia, or a lung with large bronchiectatic cavities may be a phthisis, but that alone is tubercular phthisis in which the tubercular bacillus is found.

One of the questions which, perhaps, will be most often discussed is, how does this discovery tally with the well known hereditary predisposition to phthisis, which exists in certain individuals. One can scarcely believe that a man is born with these organisms, in a latent state, already in the system, and ready to be called into activity upon the first opportunity, yet we are not without analogy that may, I think, justly be referred to in this regard. It is well known that an infant may be born apparently healthy, and in a few weeks or months present all the signs of syphilis. I do not think, however, that it is in the least necessary to suppose for a moment that the tubercular virus is actually born with the patient. It is far more likely that he is born with his tissues (in the case of phthisis, especially the lungs) in a peculiarly vulnerable condition, and thus liable to be speedily attacked and destroyed by the organisms. Here the question arises, Can this vulnerability be acquired? All evidence seems to show that it can. At this point, however, it will be asked, Is any special vulnerability of the tissues necessarily required before the virus can produce its effects? Inoculation experiments would seem to say no. The experiment animals were presumably healthy, and yet, when inoculated, tubercle was the inevitable result. It may be urged that certain animals are more liable to tubercle than others, but then this applies to a whole class of animals, and so is not of so much importance when applied to individuals. On experimental evidence alone, then, it would almost seem as if no special predisposition were required. In the case of man, however, the question is by no means so simple as this. It is well known that hundreds of individuals are daily exposed to the virus who never become affected in any way with tubercular disease, so that it would seem to be essential that some weakened state of the lungs must be present before the virus can operate. We are, therefore, forced to face the question, what is this condition of the lungs which renders them liable to be injuriously affected by the tubercular virus? Of interest, in this regard, is a paper, which appeared in the *British Medical Journal*, for 3rd February, 1883, p. 193, by Dr. Green of London. He says, "It must, I think, be admitted that something more is required than the mere presence of the organism." As I have said, inoculation experiments would seem to show that nothing more is required. He then points out, what we have already referred to, that many people are exposed to the virus who do not become phthisical. Having thus come to the conclusion that a predisposition is necessary, he says,—"It seems *probable*, I think, from this brief survey of our old pathology, that one of these conditions, at all events, is the presence of some inflammatory products within the pulmonary alveoli," . . . . and "that two conditions, at all events, are necessary in order to produce the disease: the presence of the tubercle bacillus, and some abnormal state of the pulmonary tissue with which it comes into contact—an abnormal state obtaining in the very great majority of cases in the highest portions of the lungs, and probably depending, for the most part, upon inherited or acquired constitutional feebleness." In the majority of these remarks I quite agree with Dr. Green; but I am doubtful if very much weight can be attached to what he says about inflammation as a predisposing agent. We know beyond all doubt that the tubercular virus is capable of exciting changes which are closely analogous to inflammatory changes, *e.g.*, tubercle, or catarrhal pneumonia. We also know that it is a comparatively rare thing for tubercular phthisis to ensue on croupous pneumonia, even when frequently repeated in the same patient. It may be urged that in pneumonia the inflammatory change is too acute and severe, but even after resolution the lung tissue must be in a feeble condition for a lengthened period. And in this regard I would like to mention to you a case at present in Dr. Gairdner's wards, where there have been many attacks of pneumonia, several of them in the apex, without phthisis supervening. In this case I have examined the sputum twice at intervals of a fortnight, and failed to find the bacillus. I do not wish to lay very much stress upon this, as the bacillus may possibly have escaped detection; but the patient is fast getting well, and his expectoration is disappearing. I agree much more with Dr. Green in what he says about inherited and acquired constitutional weakness, although I am quite unable to say what this may be. So far as we are entitled to go, these remarks may, I think, be taken as representing the present state of our knowledge as regards hereditary and acquired phthisis. Apart from all this, however, I think we are bound to attribute to the virus the power of producing the disease without any predisposition, if it is taken into the system in great abundance, or in a specially virulent form. The apical distribution, too, of phthisis may perhaps be explained by the supposition that the apex, being the least active part of the lung, is the situation where the virus will be least disturbed, and thus left more at rest to propagate itself and produce its disastrous results. Koch's observation, already referred to, that the bacilli require very prolonged contact, is in favour of this view. Dr. Green, in the paper already referred to, expresses a different opinion. He thinks that the bacilli flourish most luxuriantly in the apices because they are more liable to low forms of inflammation than other parts of the lungs are.

As regards the mode by which the bacillus gains access to the lung, there can be very little doubt that in the great majority of cases it is by the respired air.

In connection with this subject there are many other points of great pathological interest upon which your time will not permit me to enter. Chief among these, perhaps, may be mentioned the exceeding rarity with which general tuberculosis follows upon tubercular phthisis. In this regard, Dr. Coats has pointed out that it is, perhaps, more common than is generally supposed, to find miliary tubercles in the liver and kidneys in cases of phthisis, and to these situations the bacilli can only be supposed to have obtained access by means of the blood. In the case of the liver the virus may quite well get into the radicles of the portal vein from tubercular ulcers in the bowels, but to the kidneys it could only obtain access by means of the general circulation. The latter observation shows that the virus must frequently be in the blood, and this makes it still more difficult to understand why general tuberculosis does not more often ensue.

I have thus, gentlemen, endeavoured to draw your attention for a little to a few of what appear to me to be the more important points of interest in connection with this great problem. You will have noticed that my remarks have been almost entirely confined to the pathological aspect of the question; but I think you will agree with me that, whether viewed from the pathological or the clinical standpoint, Koch's discovery marks out a new era in our knowledge of phthisis pulmonalis.

MINUTE ORGANISMS AND THEIR RELATION TO DISEASE.

BY ROBERT MUNRO, M.A., M.D., KILMARNOCK.

PART I.

DOWN to about the middle of the seventeenth century it was universally believed that the grubs, maggots, and other small creatures, so profusely ushered into being at the beginning of each summer, originated in the substances in which they appeared, without the intervention of pre-existing organisms or parents. This doctrine, popularly known under the name of Spontaneous Generation, had the authority of Aristotle, Plutarch, and other philosophers of antiquity to commend it, and even Lucretius, whose atomic views Professor Tyndall so highly lauded for philosophical accuracy in his celebrated address to the British Association at Belfast, says, "With good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun."

It appears that Francesco Redi, an Italian physician of the seventeenth century, was the first to inveigh against the soundness of this doctrine, which he attacked on the ground that maggots could only be developed in putrid meat in which flies had previously deposited their eggs. This he proved by simply surrounding the meat by fine gauze which admitted the air but excluded the flies. Under these conditions it was found that no maggots ever appeared, and hence he justly contended that mere putrefaction could not generate the maggots. The success of these and similar experiments led Redi (1668) to maintain the theory that all living matter sprung from pre-existing living matter—a theory to which Professor Huxley, in his address to the British Association in 1870, gave the name of *Biogenesis.*

But Redi's experiments, though a sufficient refutation of the doctrine of spontaneous generation as it was then understood, soon became worthless in face of the greatly extended area of biological phenomena which followed the introduction of the microscope and the discovery of animalculæ in animal and vegetable infusions; hence ensued a controversy which has divided the learned world ever since, and only reached its acme of intensity within the last few years. It would be unprofitable, besides occupying too much space, in a sketch like the present, to attempt an exposition of the various arguments and experiments by which these controversialists supported their respective views during the century and a half which preceded the year 1836, when Schulze took up the inquiry. By this time it had been fairly established that no animalculæ appeared in infusions when boiled and hermetically sealed so as to exclude the atmosphere. The inference derived from this phenomenon by the opponents of spontaneous generation was, that the invisible germs of these animalculæ were thickly floating in the atmosphere, and that it was in consequence of their complete exclusion that no life appeared in hermetically closed up infusions. On the other hand, its advocates asserted that a free communication of the oxygen of the air was essential to vitality, and that the non-appearance of the animalculæ in the infusions was due to its want, and not to the exclusion of the germs.

The problem which thus fell to Schulze and occupied his attention was, the devising of some means by which atmospheric air previously purified of all organic matters could be admitted to the sterilised infusions. This he succeeded in doing by causing a current of air to pass through glass tubes having bulbs containing strong sulphuric acid, so as to arrest and destroy any germs that might be suspended in it. In the following year, 1837, Schwann, a Berlin scientist, contrived a method by which the air in the course of its passage to the infusion was subjected to intense heat, by causing it to pass through red-hot glass tubes, a process which could not fail to completely burn up any organic matter that it might contain. The result of both these experiments was that, as long as the above conditions were adhered to, no living organisms were found in the infusion; but, on the other hand, when the ordinary air was admitted they abundantly appeared in a few days.

By repeating and varying his experiments Schwann came to the conclusion that putrefaction was due to "decompositions of organic matter, attendant on the multiplication therein of minute organisms," which were derived from the air. He also made the important generalisation that putrefaction never takes place in a decoction of meat if sufficiently screened from the atmosphere—the truth of which is now practically demonstrated on a large scale in the modern trade of preserving meat in hermetically closed tins.

The importance of these investigations was hardly realised by scientific men, when a Frenchman, of the name of Cagniard de la Tour, announced a discovery which gave quite a new aspect to the chemistry of organic substances. This observer found that yeast was composed of small corpuscular bodies, which rapidly reproduced themselves, and so caused the long and well known phenomenon of fermentation. These newly discovered organisms were soon proved, on being subjected to careful microscopical examination, to be a low form of fungus, to which the French botanist, Turpin, gave the name of *Torula Cerevisiæ*—in short, each corpuscle turned out to be a single-celled plant. This discovery, though made almost simultaneously by Schwann and Kützing in Germany, was not universally accepted by contemporary scientists, for amongst its opponents we find two of the most renowned chemists of the day—viz., Berzelius and Liebig, who, adhering to the chemico-physical theory then in vogue, maintained that the yeast plant was only a concomitant and not an actual participator in the process of fermentation.

To the views of these chemists a most satisfactory reply was given by Professor Helmholtz in 1843. By ingeniously interposing a membrane between a fermenting and a fermentable liquid, which admitted of an interchange of the liquids, but not of their contained solids, he showed that fermentation could not be set up in the fermentable liquid by the mere passage into it of the soluble portion of the adjacent liquid, though in an active state of fermentation. Hence, he inferred that the cause of the development of these organisms—the "*primum mobile*" of the change—was resident in the solid organic particles found in all fermenting fluids. At any rate, he justly concluded that it was peculiar to something which could not pass through thin membranes.

Further experiments, notably those of Schroeder and von Dusch, by which it was shown that these infusions could be kept sterile by simply filtering the air through cotton wool, continued to strengthen the theory of biogenesis, till the year 1859, when the publication of a remarkable book by F. A. Pouchet, Director of the Museum of Natural History at Rouen, startled the scientific world by the learned ingenuity and variety of experiments with which the doctrine of spontaneous generation was advocated, or as it was called by him, "Hétérogénie." The beautiful experiments on which Schulze and Schwann so confidently relied, as well as numerous others, were found, when re-performed in the hands of Pouchet, to indicate results totally opposed to their conclusions; and the old objection, that no one had ever seen these atmospheric germs which, as he said, ought to darken the heavens if they existed so profusely and widely as was asserted, was again urged with triumphant effect. It was at this stage, when the contradictory testimony of the experimental evidence advanced on both sides necessitated a thorough revision, that the now famous *savant*, Pasteur, appeared on the scene. Originally a chemist and pupil of Dumas, this young Frenchman commenced a series of experiments, which not only gave the final *coup de grace* to the hydraheaded question of spontaneous generation, but opened up other fields of inquiry which have already yielded most valuable results.

Pursuing the method of filtering the air by means of plugs of cotton wool inserted in the mouths of the tubes through which the air was admitted into the infusions, Pasteur demonstrated by a microscopical examination of the particles, thus arrested in the meshes of the cotton wool, the existence among them of numerous bodies which he recognised as germs, and which, when sown in previously sterilised infusions, were always found capable of giving rise to living organisms. Moreover, by using glass flasks having their mouths greatly drawn out and bent downwards, he found that the cotton plugs might be entirely dispensed with, without in the least interfering with the sterility of the contents of the flasks. The explanation he gave of this important fact was, that the germs, being of a particulate or material character, had a tendency to settle downwards, and, in a perfectly still atmosphere, never mounted upwards through the tubes, thus proving that the free interchange of air did not originate life.

By these and many other ingenious experiments, an account of which was published in 1862, Pasteur confirmed the accuracy of the results of many of the previous experiments, when performed with care, which had been called in question by Pouchet and his followers, and came to the final conclusion that no living organism, however low in the scale of life, ever appears without the intervention of antecedent life.

The end of this long chain of experimental evidence is the starting point of a new departure in medical research, the ultimate goal of which is no less than the complete expulsion of all epidemic diseases. Before, however, diverging into these new paths, there are a few observations, gleaned from the subsequent phases through which the controversy regarding the *de novo* origin of life has passed, which, though only corroborating the main conclusions arrived at by Pasteur, demand our attention on account of the additional light they throw on the methods by which the higher and more interesting problems we are shortly about to discuss are being wrought out.

Our illustrious countryman, Professor Tyndall, describes in the following words how he became acquainted with a simple but effectual method of testing the purity of air :—

"During the ten years extending from 1859 to 1869, researches on radiant heat, in its relations to the gaseous form of matter, occupied my continual attention. When air was experimented on, I had to cleanse it effectually of floating matter, and while doing so I was surprised to notice that, at the ordinary rate of transfer, such matter passed freely through alkalies, acids, alcohols, and ethers. The eye being kept sensitive by darkness, a concentrated beam of light was found to be a most searching test for suspended matter both in water and in air—a test, indeed, indefinitely more searching and severe than that furnished by the most powerful microscope. With the aid of such a beam I examined air filtered by cotton-wool, air long kept free from agitation, so as to allow the floating matter to subside, calcined air, and air filtered by the deeper cells of the human lungs. In all these the correspondence between my experiments and those of Schroeder, Pasteur, and Lister, in regard to spontaneous generation, was perfect. The air which they found inoperative was proved by the luminous beam to be optically pure, and therefore germless."

Armed with this luminous beam, as a magic wand, Professor Tyndall could at once detect the slightest impurities in air, and even predict where infusions would remain sterile. By it he discovered that an open flask, containing common air from his laboratory, always showed a marked luminous track ; but, if this same flask was turned with its mouth downwards and left undisturbed for a few days, the course of the beam was not at all visible. He then devised a still chamber, with various ingenious contrivances, in which he experimented on many different infusions with results entirely corroborative of Pasteur's great generalisation. On one occasion he packed sixty hermetically sealed flasks, having their greatly elongated mouths tapering to fine points and bent downwards, and containing sterilised infusions of beef, mutton, turnip, and cucumber, and transported them to his mountain home on the Bel-Alp, at an altitude of some 7,000 feet, and overlooking the great Aletsch Glacier. When the case was unpacked, fifty-four of the flasks were found to be clear as water, but the remaining six had become turbid. On careful inspection of the muddy flasks—the muddiness of course indicating that the liquid was swarming with living things—it was found that the fine tips of the glass tubes were broken off in the course of their transit, and so permitted air to rush in. The remaining fifty (four having been accidentally broken) were exposed to the warm Alpine sun by day, and at night suspended in a warm kitchen. At the end of a month they were as clear as ever and showed no sign of putrefaction though the season (July and August) and other conditions were most favourable to putrefactive changes. But the rest of this experiment is best told in the Professor's own words.

"The question now is, Whether the admission of air can liberate any generative energy in the infusions? Our next experiment will answer this question, and something more. We carry the flasks to a hayloft, and there, with a pair of steel pliers, snip off the sealed ends of the group of three-and-twenty. Each snipping off is, of course, followed by an inrush of air. We now carry our twenty-seven flasks, our pliers, and a spirit-lamp, to a ledge overlooking the Aletsch Glacier, about 200 feet above the hayloft, from which ledge the mountain falls almost precipitously to the north-east for about a thousand feet. A gentle wind blows toward us from the north-east—that is, across the crests and snow-fields of the Oberland mountains. We are, therefore, bathed by air which must have been for a good while out of practical contact with either animal or vegetable life. I stand carefully to leeward of the flasks, for no dust or particle from my clothes or body must be blown towards them. An assistant ignites the spirit lamp, into the flame of which I plunge the pliers, thereby destroying all attached germs or organisms. Then I snip off the sealed end of the flask. Prior to every snipping the same process is gone through, no flask being opened without the previous cleansing of the pliers by the flame. In this way we charge our seven-and-twenty flasks with clean vivifying mountain air.

"We place the fifty flasks, with their necks open over a kitchen stove, in a temperature varying from 50° to 90° Fahr., and in three days find twenty-one out of the twenty-three flasks opened on the hayloft invaded by organisms—two only of the group remaining free from them. After three weeks' exposure to precisely the same conditions, not one of the twenty-seven flasks opened in free air had given way. No germ from the kitchen air had ascended the narrow necks, the flasks being shaped to produce this result. They are still in the Alps, as clear, I doubt not, and as free from life as they were when sent off from London." (*Nineteenth Cent.*, vol. iii, p. 32.)

Dr. Tyndall has thus established by a variety of ingenious experiments that—

(1.) Ordinary air is at all times beset with ultra-microscopical particles which communicate to it, when acted on by a luminous beam, a peculiar opalescence which disappears when the air is allowed to remain absolutely tranquil.

(2.) Air from which these ultra-microscopical particles have subsided is no longer capable of contaminating organic infusions previously sterilised.

(3.) The ultra-microscopical particles are therefore the germs of the full grown microbes.

Professor Huxley, while seeing no alternative but to adopt the doctrine of biogenesis as expounded by Pasteur, Tyndall, and others, carefully guards himself against giving countenance to the supposition that no such thing as abiogenesis ever has taken place in the past, or ever will take place in the future. On this point he uses these remarkable words:—

"If it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed like existing fungi, with the power of determining the formation of new protoplasm from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light." (*Critiques and Addresses*, p. 239).

A somewhat similar opinion is thus expressed by Professor Tyndall :—" While expressing, therefore, unshaken 'belief in that form of materialism' to which I have already given utterance, I here affirm that no shred of trustworthy experimental testimony exists to prove that life, in our day, has ever appeared independently of antecedent life." (*Nineteenth Century*, March, 1878.)

The prominence already given to the beautiful experiments which demonstrate that, within the limited horizon of human experience, we have no knowledge of organic life, except as derived from antecedent life, sufficiently indicates our own opinion. With the advocates of the theory that living organisms do appear, *de novo*, in sterile infusions, in a few days or months, under any circumstances, we have no sympathy whatever. In support of such a doctrine we know of no argument. Out of harmony with our knowledge of the laws of evolution, and, experimentally, proved to be untenable, we cannot avoid believing it to be also eminently improbable. But, on the other hand, in accepting of biogenesis as thus defined, it does not follow that abiogenesis, either in the past, present, or future, must be discarded. We can neither understand nor appreciate the bitterness of the controversy to which this problem has given rise during the past decade of this century. Professor Huxley thinks that the evolution of living protoplasm from not living matter took place in the remote past; but he seems to oppose the idea of its occurrence under present physical conditions. Sir William Thomson, in accounting for the origin of life, had recourse to some stray star-stuff—a "moss grown fragment from the ruins of another world," which, like a little leaven, leavened the whole globe. But this is pure speculation. By the way, might we ask Professor Huxley how long does he suppose he would have to wait, even with his cultured powers of observation, were it given to him to be a witness of the evolution of living from non-living matter, so as to be able to describe the processes by which this great and eventful phenomenon had been accomplished? Were we to take a selection of the inorganic substances of this globe, such as the ammonium carbonates, oxalates, &c., and the other ingredients enumerated above by the Professor, and, having previously sterilised them by heat, place them in Professor Tyndall's still chamber, under circumstances otherwise favourable to the manifestation of organic life, is there any reason why a recurrence of this wonderful phenomenon should not take place? It might not be in a few days, or a few years, or even a few million years, but the question is, would it ever take place? Who can tell what time Nature took to produce protoplasm from the smouldering embers of this globe! or who can count the ages that rolled on ere protoplasm became shaped into the smallest organism definable by human art! Again, did bacteria in the remote past spring up 24 hours after the introduction of protoplasm as they now do? As well might we maintain that in other 24 hours these bacteria had become tadpoles. Rome was not built in a day; neither was this beautiful world fashioned and peopled with such rapid strides. Whence, then, the reasonableness of the philosophy that expects the evolution of living organisms from inorganic matter, in a few hours, within the precincts of a scientist's laboratory? Let us rather seek to know something of the long series of living things that doubtless preceded the appearance of these so-called infusoria in the upward chain of evolution. Whether these simple organisms are later products of protoplasm, or degenerate forms from higher progenitors who have been outstripped by their congeners in the life struggle of evolution, we know not; but in point of time, their development may be as far distant from the date of the first appearance of the primordial molecules of protoplasm as the various birds that now so gracefully skim the air are distant from their ancient Saurian progenitor whose ærial aspirations first set the machinery in motion that has developed, in our day, such perfection in the art of flying.

We shall now go back to the year 1862, when Pasteur published his celebrated memoir in the *Annales de Chimie*, describing the experiments and researches which established the fact that the origin of these minute organisms was no exception to the ordinary laws of generation, and consequently led him to reject the theory of spontaneous generation as an unfounded "chimera."

The main conclusions then arrived at may be briefly summarised as follows:—

(1.) No organisms are developed in sterilised infusions, with or without free access to air, if sufficient care be taken to prevent the presence of germs.

(2.) The floating particulate matters found in ordinary air, when collected and introduced into solutions which had previously remained sterile, are capable of giving rise to organisms.

(3.) Putrefaction is due to the developmental action of very minute living bodies, called bacteria, whose germs are always floating in ordinary atmosphere.

(4.) All ferments are the offspring of living things, similar to themselves, which never appear in any liquid without their germs having been previously introduced.

(5.) The thermal death point of different germ particles is not the same. Germs, when dried up by floating in the air, not only retain their germinal power for unlimited periods, but may be subjected with impunity to influences that would otherwise destroy their vitality, such as a high or low temperature, strong acids, alkalies, &c.

About this time the frequent and disastrous losses sustained by the winegrowers and vinegar makers in France assumed such a magnitude as to materially affect the commercial prosperity of the country. The finest wines often turned sour or bitter, in a most unaccountable manner, and, there being no certainty that this change would not take place after exportation, the wine trade became greatly restricted. To the investigation of the cause of the wine disease, which had now become a question of national importance, Pasteur directed his energies with the most satisfactory results. The deterioration of the wine he traced to the fermenting products of other organisms, besides those of true fermentation, which he discovered clinging to the skins of the grapes. It was during this investigation that Pasteur explained the reason why fermentation apparently occurred in the juice of the grape without the addition to it of yeast, as is always done to the juice of barley. From microscopic examination of the dust covering the skin of the grape, as well as various experiments made with this dust and juice extracted antiseptically from the interior of the grape, he proved that yeast was as essential to the fermentation of wine as that of beer, but that its artificial introduction into the former was unnecessary as it was always a concomitant or parasite of the grapes themselves. The simple but effectual remedy which Pasteur suggested for this disease was to heat the juice of the grapes up to a temperature of 50° Centigrade,—a degree of heat which, he had ascertained, would kill the spurious ferments without damaging the wine.

Pasteur then passed on to inquire into the adulteration or putrefaction of vinegar, and, having ascertained that the *mycoderma aceti,* or vinegar plant, was also associated with spurious ferments whose products vitiated the true acetous fermentation, he similarly showed that by their destruction the health of the vinegar was restored.

From such careful study of the process and products of fermentation Pasteur was led to grapple with the pathology of beer, which ended in the introduction of some important improvements in the art of brewing. By his usual scientific methods he proved that every diseased alteration in the quality of beer was coincident with the action of microscopic organisms other than true yeast. Hence, by boiling the wort, so as to kill all the fermenting organisms, and allowing it to cool under conditions that precluded their re-entrance, and afterwards fermenting it with pure yeast, a high-class beer was produced. Beer was found to be assailable by all the various organisms which produce acetic, lactic, or butyric acid, all of which destroy the flavour of this beverage, and even the yeast itself was frequently attacked by the bacteria of putrefaction. Hence, if a brewer wishes to have good beer these so-called ferments of disease must be strictly excluded or drugged into a condition of inactivity. According to Pasteur alcoholic fermentation was a chemical act, attendant on the life of cells when removed from the free action of oxygen and submerged in a saccharine solution capable of yielding oxygen and heat by decomposition. Hence the alcohol-producing power was not exclusively the property of the true yeast plant *(torula cerevisiæ),* but belonged, though in a minor degree, to all living cells when submerged and unable to obtain oxygen otherwise than by the decomposition of their surrounding liquid.

In 1876, Pasteur published his valuable work *Etudes sur la bière,* a worthy sequel to his two previous popular summaries of *Etudes sur le vin*, and *Etudes sur le vinaigre,* in all of which he has effected improvements that have conferred lasting benefit on all interested in the success of these industries.

But these were not the only investigations of national importance which engaged the attention of this distinguished chemist. In consequence of a terribly destructive plague, which raged among the silkworms of France and Italy, the weight of cocoons in the former country, which amounted in 1853 to about fifty-two million lbs., was reduced in 1865 to eight millions, thereby entailing a loss, in the last mentioned year alone, of four million pounds sterling. During these disastrous years, chlorine, sulphurous acid, nitric acid, quinine,

&c., together with a host of the usual medical quackeries were vaunted as effectual cures and showered upon the impoverished sericulturists. Even Government came under an obligation to pay half a million francs for a remedy which its promoters declared to be infallible. Upon repeated trials, however, this vaunted remedy proved a complete failure. It was in these circumstances, and at the instigation of his friend and former master Dumas, that Pasteur undertook to investigate into the cause of this disease. Accordingly, in June 1865, he retired to Alais, one of the chief centres of the silk industry, and the native place of Dumas, in order to avail himself of every possible advantage while studying the phenomena of this peculiar and most intractable scourge. It was called pébrine, on account of certain black spots which frequently covered the bodies of the affected worms, and its most common symptoms were languor, failure of appetite, stunted appearance, and premature death.

Microscopical observations had already shown that the intestinal canal, blood, and tissues of diseased worms, were permeated with peculiar vibratory corpuscles. These corpuscles, which were of a cylindrical shape, filled the silk bag, and thus occupied the place of the clear viscous substance from which the silk was spun. They were found in the animal in all its cyclical stages of worm, chrysalis, and moth; but whether they were the cause, or mere concomitants of the disease, was still a disputed problem. Believing that they could distinguish healthy from diseased eggs, the Italian naturalists founded a practical method of counteracting the ravages of the pébrine by subjecting the eggs to microscopical examination before risking incubation. Pasteur, however, proved that the plague-corpuscles might be so incipient in both the egg and the worm as to escape detection by the microscope, but at the same time he made the important discovery that, as the animal grew larger, these corpuscles also grew larger and became so pronounced in the moth that no such difficulty of detection ever occurred. In order, therefore, to procure healthy eggs it was necessary to select healthy moths. Having secured healthy eggs his next step was to devise some means of protecting the worms thus hatched from external contagion. In the course of his subsequent observations he ascertained that those healthy worms were liable to become affected with the disease, either through germs in their food or by contact with diseased animals, but that, in this case, the disease never took such hold on them as to prevent them spinning their cocoons—and hence that the crop of silk for that season was at any rate safe. It was therefore only when the disease became hereditary that it interfered with the silk-producing function of the animal. Hence the protective method which Pasteur recommended, at the meeting of the Academy of Science on the following September, was to see that the eggs for incubation were always procured from healthy moths. These views at first met with great opposition. Pasteur, however, in order to overcome all prejudice as speedily as possible, resorted to the expedient of prophesying, from an inspection of fourteen parcels of eggs and the parent moths, what the result would be in the following season. The correctness of his predictions, which were kept in sealed envelopes till the proper time, led to the immediate adoption of his method of selecting healthy worms, the consequence of which was that the silk industry of France was speedily restored to its former prosperity. Wishing to make his investigation as complete as possible, he subjected the deadly corpuscles, in all stages of their development, to a searching examination, and thus traced the various modes in which the plague was propagated. Rubbing up a diseased worm with a little water he smeared the mixture on the leaves on which healthy worms were feeding and found that every one that ate of it became affected with the disease. He also ascertained that the worms inoculated each other by scratching with their claws. Even the alvine dejections of diseased worms contained infectious germs which, adhering to the leaves, spread the disease among the worms that subsequently fed on these leaves. In 1870 Pasteur published his work *Sur la maladie des vers à soie*, in which he explained the successive steps of his investigation, and established, with singular completeness, the success of his protective measures which, it is now universally acknowledged, actually saved the silk industry from utter destruction. Professor Huxley estimates the direct loss to France caused by the pébrine, during the 17 years previous to the adoption of Pasteur's method of extirpating the germs of this plague, at fifty millions sterling.

(*To be continued.*)

NOTES OF A CASE OF ENTERIC FEVER WHICH HAD TWO RELAPSES, WITH AN UNUSUAL PROLONGATION OF THE INTERVAL BETWEEN THE FIRST AND SECOND ATTACKS. (WITH CHART OF TEMPERATURES.)

By ROBT. W. FORREST, M.D.

(*Communicated to the Glasgow Pathological and Clinical Society, 13th March, 1883.*)

This case was one of four which occurred in the same household, during an outbreak of the disease in Crosshill, Glasgow, about the autumn of last year. The first patient was seized towards the end of July, the present case was the next, the third began about the 10th of August, and the fourth in the beginning of December. The last was believed to be an instance of direct infection, from nursing the second case. They all recovered.

G. R., æt. 19, male, was first seen on the morning of 1st August; he had been feeling unwell for about a fortnight, not taking food as usual, yet not ill enough to stay from business, and he would have gone out as usual on that day, if I had not advised him to remain at home. His temperature was then 100°; it rose in the manner fairly typical of primary typhoid till five days later, corresponding probably to the sixth of the attack, when it had reached 105°, falling next day to 104°, and three days later to 103°, about which level it remained six or seven days, falling after that pretty steadily, and getting fairly below 99° by the evening of the thirtieth day.

The temperature remained about normal, or subnormal, for thirty-two days, excepting on the evening of the twenty-second day of apyrexia, when it rose to 100·2°, due, it was thought, to his remaining too long out of bed; it then began to rise again, reaching 103·8° on the evening of the fifth day, about which it remained till the evening of the ninth day, when it ran up to 104·8.° From this point it fell gradually till, on the evening of the twenty-third day, it reached 98·4°, about or below which it remained for seven days, when for the third time it began to rise; it got to 104·2° on the fourth and 104·3° on the fifth day, remaining about this level other two days. It then rapidly fell, reaching 99° on the evening of the thirteenth day, where it remained for another week; and then normal and subnormal temperatures were recorded, and continued till convalescence was completed.

During the first period of pyrexia, the pulse ranged from 70 to 100; during the second and third periods from 74 to 104.

During the first interval of apyrexia, the pulse was from 50 to 70; during the second interval and the early part of the convalescent period, from about 50 to 60.

A few rose spots were observed during each attack. The bowels were confined throughout, and were relieved daily by small doses of castor oil till the beginning of the second week of the first apyrexial interval, when a few drops of blood were observed in the stools; simple enemata were then substituted for it, and were continued till the end of the illness; blood, however, in small quantity, continued in the stools till the middle of the apyrexial interval. A few days later on patient began to get up, and continued out of bed the greater part of the day, but had not been out of doors when the second attack set in.

A little blood was again observed in the stools during the decline of the fever, and for six days of the apyrexial interval following the first relapse, and on one occasion, on the twelfth day of the second relapse, when the temperature was 99·4°. Profuse sweatings occurred on several days during the second relapse. (See chart.) Fluid food only was given during the primary attack, a little arrowroot was then allowed, and during the latter half of the first apyrexial interval a little fish and potatoes were given at dinner. Fluid food was resumed when fever set in again, and was continued till convalescence was firmly established.

Quinine, in three grain doses, to the extent of 15 to 18 grains daily, was given during the second and third periods of pyrexia; other than its usual physiological effects, it produced no inconvenience, and was thought rather to maintain the patient's ability to take milk, &c., freely. In spite of it, however, the relapses followed pretty closely the course generally found in relapses, the temperature, notably in the second relapse, rising to its highest level more quickly than in the primary attack, and bearing out the observation of Pearson Irvine, that the highest temperature in relapses is met with on the fifth day,* and that in the great majority of instances the rise to the fifth day is all but uninterrupted.† The record also supports his view, that in cases of relapse, if normal in

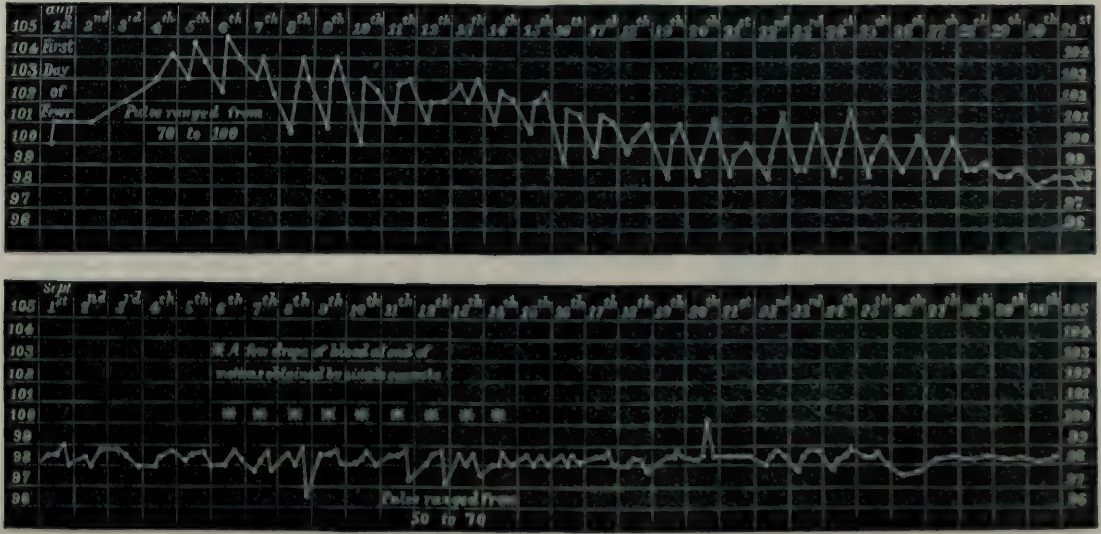
---

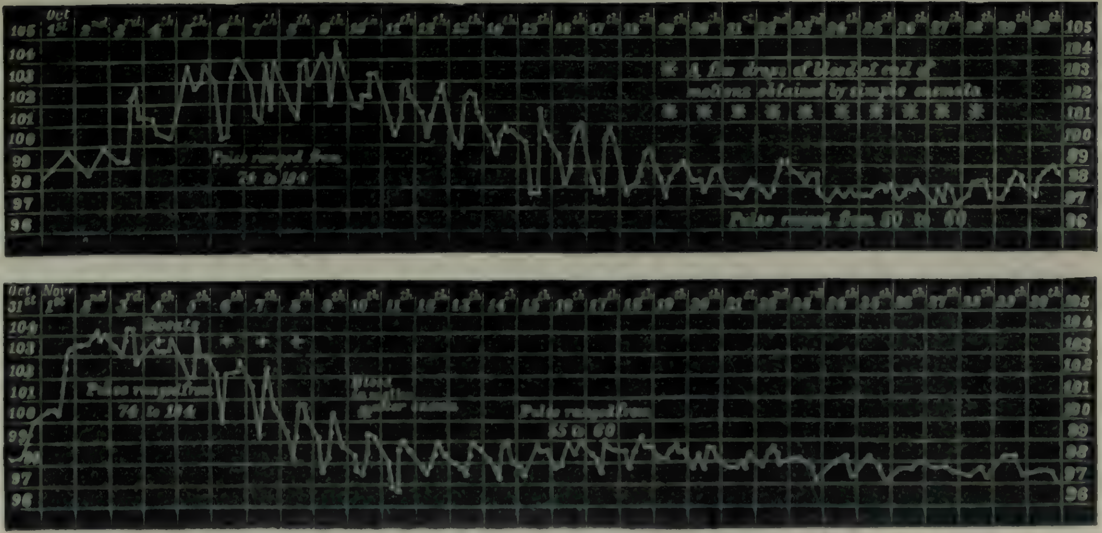
* J. Pearson Irvine: *Relapse of Typhoid Fever, especially with reference to the Temperature.* London, 1880. P. 119.

† *Loc. cit.*, p. 132.

TEMPERATURE CHART OF A CASE OF ENTERIC FEVER, CHARACTERISED BY TWO RELAPSES, WITH AN UNUSUALLY LONG INTERVAL BETWEEN THE FIRST AND THE SECOND ATTACKS.

The Patient was a Young Man of 19 Years.





Temperatures taken in mouth at the times indicated on the chart.—A few "rose spots" during the three febrile periods.—No diarrhœa throughout; small doses of castor oil at first, but from 6th Sept. simple enemata used; a little blood in the motions at the times marked on chart.—Fluid food exclusively used, except for about a fortnight in the second half of Sept., when a little fish and potatoes were given.—Patient was confined to bed, except during the last fortnight of Sept., when he was up, but never out of doors.—Quinine, 15 to 18 grs. *per diem*, given during the febrile period of both *Relapses*.—Sweatings during second relapse, at points marked on chart.—Recovery complete and satisfactory, the temperature having been noted daily for some time after the dates in the chart.

their course, a fall occurs about the eighth day,* while in primary typhoid the temperature retains till the twelfth day nearly, though not quite, the maximum attained in from the fourth to the sixth days.† This fall about the eighth or ninth day, in the absence of hæmorrhage and other accidents, he looks upon as a most valuable and favourable prognostic indication,‡ he thinks that the eighth and ninth days' temperatures in typhoid relapses have a critical significance.§

The specially interesting feature in this case is the unusual duration of the interval between the first and second attacks. Murchison gives statistics of relapses with eighteen and nineteen days' intermissions, and one of twenty-five days. In Pearson Irvine's cases three had intervals of ten days. Dr. Allan, of the City of Glasgow Fever Hospital, records a case‖ which had two relapses, with sixteen days between the attacks. His case was a very interesting one, and had quinine administered freely, without any clear evidence of benefit from its use.

Pearson Irvine seems to doubt that relapses of typhoid may set in so long after primary typhoid, as happened in this case. He says:¶—"I am convinced that in many cases of deferred relapse a careful daily observation of the temperature would have shown that the patients were victims of repeated relapses, possibly exceedingly mild, but still determinable by the thermometer." ** "It is difficult to believe that a recurrence of a typhoid attack, weeks or even months after a patient's convalescence, is due to the primary cause of the disease."

This interesting question remains: Whether was the first relapse due to the primary contagion—as is most probably the case when relapse sets in shortly after the termination of the previous attack, and possibly having something to do with the constipation or with the ulceration, of which the bleedings recorded in the chart are the signs—or whether was it due to a new infection? The probable cause of the outbreak was investigated by Dr. Duncan, who ascertained that the majority, if not the whole of the patients, had been using milk from one source, the sanitary arrangements of which were deplorably defective. The dairy which supplied milk to this household was supposed to sell no milk other than that furnished by its own cows; but it was subsequently ascertained on inquiry that, when their own supply threatened to run short, the dealers obtained the extra milk they required from this same source. When this was discovered, the doubtful milk was discontinued, but whether sufficiently early to make any difference to the patient I cannot ascertain. The house drains were found in a tolerably good state, but were carefully overhauled and ventilated long before the termination of the primary attack.

---

* *Loc. cit.*, p. 119.

† *Loc. cit.*, p. 132.

‡ *Loc. cit.*, pp. 119, 132.

§ *Loc. cit.*, p. 120.

‖ *Glasgow Med. Journal*, May 1881, p. 363.

¶ *Loc. cit.*, p. 112.

** *Loc. cit.*, p. 136.

ON BI-CHROMATE OF POTASH POISONING.

By EDWARD ORR MACNIVEN, M.B. & C.M.EDIN., House Physician in the Glasgow Royal Infirmary,

B. D., æt. 22 years, workman in Mr. Carlyle's chrome works, Garngad Hill, was admitted into Dr. Maclaren's Wards in the Royal Infirmary, on the 10th December 1882, suffering from the effects of swallowing a quantity of bi-chromate of potash.

History.—The patient, a fairly muscular man, who had fallen in love with a young woman who did not return his affections, in a freak of jealousy went to the chrome works about five o'clock on Sunday evening and swallowed a lump of chrome (the purified salt) in the solid form. He then returned to his lodging, which is about fifteen minutes' walk from the works. As soon as he reached his room he experienced the following symptoms:—

The first thing he noticed was a lightness in the head; then he experienced a sensation of great heat in the stomach, with a glow of heat all over the body; this was followed by a cold sweat. Next he became nauseated and vomited freely. He then suffered from agonising pain in the epigastric region, along with giddiness, specks before the eyes, and loss of power of the legs. (He had complete power of his arms.) His thirst was intense; as he aptly expressed himself, "he felt as if he could drink the sea dry." Lastly, he complained of severe rigors, with coldness of the whole body—more especially of the extremities.

The patient was carried into the hospital at ten minutes to seven o'clock, nearly two hours after swallowing the poison; on examination I found the pupils slightly dilated, the face pale and extremely cold, the pulse feeble and fluttering. There was no vomiting then; but he complained of intense pain over the region of the stomach, and a feeling of great depression. No cramps or diarrhœa were present. There was a degree of stupor, but he answered questions fairly well. Sensibility to touch and pain was well marked.

Treatment.—This consisted in giving a full dose of sulphate of zinc; washing out the stomach with tepid water by means of the stomach pump till the fluid was colourless. As the pulse threatened to fail, I injected subcutaneously 20 mins. of sulphuric ether, which was followed by a marked improvement. The patient was covered with plenty of warm blankets, and hot bottles were applied to the feet and sides. A mustard poultice was applied over the stomach, which gave great relief to the patient. I next tried the administration of some tepid coffee, well diluted with milk, and plenty of brown sugar in it; this was rejected at once. Next I tried some milk well mixed with a good quantity of lime water, and 10 grains of subnitrate of bismuth, and this was retained. Barley water was given as a drink, and the patient was ordered a milk diet with lime water. The patient slept fairly well that night, and in the morning every symptom had disappeared, except a slight soreness of the mouth. Diet was strictly attended to as usual. The patient made a perfect recovery without a single bad symptom.

Remarks.—1. The first symptom—viz., "the lightness in the head" (apparently a tendency to syncope), took place fifteen minutes after he had swallowed the poison.

2. Had it not been (*a*) That vomiting occurred in his lodgings (which happened about twenty minutes after he had taken the poison), and (*b*) That the patient had taken food about an hour and a half before he entered the works, the effects might have been much more disastrous.

3. Although a good quantity of the poison was found in his stomach, yet the rapid evacuation of its contents, together with the application of tepid water, along with milk, lime water, and bismuth, tended to soothe the mucous membrane, and diminish the chance of inflammatory action. The application of the mustard externally had also a beneficial effect in the same direction.

4. Over ʒii, according to the [*] patient's account, must have been swallowed, and by a rough analysis of the stomach's contents, I estimated that it contained nearly ʒi of the bi-chromate. No trace of the poison was found in the urine with

---

[*] I made the patient select a piece of the bi-chromate which he thought was exactly the same size as that which he swallowed, and then I weighed it and found it over ʒii.

the ordinary tests for its detection—viz., nitrate of silver, red; acetate of lead, yellow; sulphuretted hydrogen, dingy green.

As bi-chromate of potash poisoning is somewhat rare in occurrence, perhaps a brief summary of the leading symptoms of a few cases recently recorded (comparing these with those of the case just described) may be interesting.

I. Case recorded by Dr. Maclachlan [*] of accidental poisoning, in which ʒiii were taken dissolved in water, and the first symptom appeared a quarter of an hour after administration.

Symptoms.—Excessive vomiting, purging, violent abdominal pains, with cramps in the legs, coldness of the surface of the body. Hands were shrivelled, wrinkled, and dusky, like a person in the advanced stage of cholera. Face and lips dusky, with yellowness of the conjunctiva, excessive thirst, feebleness of the pulse, hurried respiration, suppression of the urine, soreness of the mouth. Mental faculties unimpaired.

Resembles the case I have described in having the following symptoms present, viz.;—1. Vomiting; 2. abnormal pains; 3. excessive thirst; 4. coldness of the surface of the body; 5. feebleness of the pulse; 6. soreness of the mouth.

Differs from it:—(*a.*) In having the following symptoms absent, viz.—1. Tendency to syncope, with giddiness; 2. specks before the eyes, with slight dilatation of the pupil; 3. degree of stupor; 4. loss of power of the legs.

(*b.*) In having the following symptoms present, viz.:—1. Suppression of urine; 2. purging; 3. hurried respiration; 4. peculiar condition of the face and hands, with yellowness of the conjunctiva.

II. Case recorded by Dr. R. Archer Warwick [†] of intentional poisoning, in which a lump of chrome, about ℥ss in weight, was taken, and the first symptom appeared in one or two minutes after administration.

Symptoms.—(*a.*) Resembling the case I have described:—1. Pain in the stomach; 2. giddiness and vomiting, with violent retching; 3. loss of power of the legs; 4. feebleness of the pulse and clammy perspiration.

(*b.*) Differing:—Purging, with blood in the stools.

III. Case recorded by Dr. M'Crorie. [‡]

Symptoms.—(*a.*) Resembling:—1. Vomiting; 2. dilatation of the pupils; 3. feebleness of the pulse; 4. tenderness of the abdomen; 5. coldness of the surface of the body, with clammy sweat.

---

[*] *Glasgow Medical Journal*, July 1881, page 31.

[†] *Lancet*, 31st January, 1880, page 167.

[‡] *Glasgow Medical Journal*, May, 1881, page 378.

(*b.*) Differing:—1. Purging; 2. difficult wheezing respiration.

IV. Case recorded by Dr. Dunbar Walker[*] of accidental poisoning, in which ʒi was taken dissolved in water, and the first symptom appeared in five minutes after administration.

Symptoms.—(*a.*) Resembling:—1. Sickness; 2. tendency to syncope; 3. coldness of the body; 4. pain in the abdomen; 5. feebleness of the pulse.

(*b.*)—Differing:—Purging.

Judging, therefore, from the cases just mentioned, and from those of an earlier date,[†] the symptoms produced by bi-chromate of potash are those of an irritant poison, and may be described as follows:—

(*a.*) *General or Usual Symptoms.*—1. Abdominal pains. 2. Vomiting. 3. Purging. 4. Cramps in the legs. 5. Feebleness of the pulse. 6. Coldness of the surface of the body, with clammy perspiration, soreness of the mouth and throat, and excessive thirst.

(*b.*) *Remote Symptoms.*—1. Tendency to syncope, with giddiness—specks before the eyes, dilatation of the pupils, and yellowness of the conjunctiva. 2. Suppression of the urine, stupor, hurried respiration, and loss of power of the legs.

The accurate experiments on dogs with this substance, performed by Professor Gmelin,[‡] of Tübingen, are also worthy of note, as illustrating the valuable information to be derived from such investigations.

Lastly, the local effects on workmen who use the bi-chromate of potash in dyeing, as described by Drs. Duncan[§] and Eadie[‖] of Glasgow, are of great practical interest, and illustrate the caustic action of this poison. It seems that on those workmen who have the slightest abrasion of the skin, the poison acts as a caustic, producing a tough slough, followed by an ulcer with hardened, cup-like border. These sores may gradually extend deeper and deeper, until they eat their way into the bone; and sometimes they actually make their way through the arm or hand altogether. Attacks of conjunctivitis are also of not uncommon occurrence. These observations I can fully confirm.

---

[*] *Lancet*, 27th September, 1879, page 464.

[†] Taylor's *Medical Jurisprudence*, page 127. Woodman & Tidy's *Handy Book*, page 172.

[‡] Christison on *Poisons*. 2nd edition, page 444. *Bulletins des Sciences Méd.*, xx, p. 188. From the *Heidelberg Klinische Annalen*.

[§] *Edin. Med. and Surg. Journ.*, xxvi, p. 133.

[‖] Guy's *Forensic Medicine*, p. 510.

A NEW DRESSING FOR LARGE ULCERS.

By ARCHD. PEARSON, M.D.

Read before the Glasgow Southern Medical Society, 8th March, 1883.)

Mr. President and Gentlemen,—I wish to draw your attention very briefly to a few notes on a new dressing for large ulcers, which I may call a dry dressing. I know the dry dressing has been tried by several before now, but not in the same way I am about to bring before you to-night. Some two years ago, I was called to see an old maiden lady with a large, dry, gangrenous ulcer on the shin of the leg, two inches above the ankle, nearly encircling the leg, and about four inches broad. After getting the sloughs removed, I used a lotion of boracic acid with lint, dressing in the ordinary way. I may mention that I could not get my patient to take the necessary rest which her case required—move about she would. The removing of the dressings, even with the greatest care, produced much pain.

I continued this style of dressing for about four weeks with no indication of healing. Seeing the great irritation produced by the very finest lint on removing it, I thought of trying something less irritating, such as some folds of Indian muslin moistened with boracic acid lotion next the ulcer, and over this, a fold of dry lint, on which I spread oxide of zinc ointment, over this again two folds of dry lint, and over all Mackintosh and bandage. I removed this dressing on the third day by a stream of water from a large teapot, and to my patient's delight she felt little or no pain. Previously, the removing of the dressings was her greatest dread. This kind of 'dressing I continued for about three weeks with considerable healing all round, and great relief to the patient's feelings. At this stage I laid aside the oxide of zinc ointment, and substituted boracic acid lint in *three dry folds*, retaining the muslin, as before, next the sore, and covering all with Mackintosh and bandage. These dressings were allowed to remain for three days, and carefully removed under a stream of water, and latterly the sore was cleaned with boracic acid lotion before applying the dressings. This large and unhealthy looking ulcer was completely healed in about ten weeks from the time the dry dressing began. Since then I have used the same style of dressing on smaller ulcers with like success.

Within the last few months I put this dressing through a crucial test. The little patient I bring before you to-night had the misfortune to be run over by one of the Ibrox cars on the 10th of August, 1882, receiving a comminuted fracture of the right thigh bone, compound dislocation of left knee, with laceration of the skin and flesh of the anterior part of the left thigh.

Dr. Hector C. Cameron, who saw him first with me, thought he could hardly survive the shock. He only saw him once, as he was leaving for his holidays that evening; but Mr. Clark took up the case, and kindly attended with me until the sloughs were nearly removed. In passing, I think it not out of place to mention the great benefit the little patient derived from nepenthe during the three days the shock lasted. He got ten drops every few hours, as the symptoms required. Its soothing effect was shown by the fact that on the third day after the accident, when the first dressings were removed, he was as quiet as if he had been under the influence of chloroform, though that anæsthetic was not used. The dressings at first were the carbolic antiseptic, until the sloughs were removed. After they were removed, nearly the whole anterior and inner aspects of the thigh, down to the knee, were one entire surface of raw flesh. Sanguine as I was of the *dry dressing*, I had my doubts if I could get such a surface to skin over. If possible I would assist it by grafting—this being a good case for it, so I took a good large graft from the arm of the boy's father and placed it whole about two inches above the lower edge of the sore. It adhered and retained its vitality, and my hopes were raised as I had a good large island of skin to start from. A few days thereafter Mr. Clark called to see the patient, and took a graft from the arm of the nurse and cut it into small seedlets, inserting them into the upper edge of the sore. I repeated grafts from the arm of the father and nurse twice afterwards, inserting them into the outer and inner sides. All the grafts retained their vitality for weeks, but showed no appearance of enlarging or throwing out skin. For about three weeks while these graftings were tried, the dressing consisted of the boracic acid lotion with the muslin, oxide of zinc ointment spread on dry boracic acid lint with other two dry folds of it, the whole being covered over with Mackintosh and bandage, and dressed every alternate day. The sore was healing; the patient bore the removal of the dressings well; had no pain, large as the surface was. Of course great care was taken that the dressings to be removed were well soaked with water. As in the first case mentioned, I laid aside the ointment about the third week, retaining the muslin and three folds of dry boracic acid lint, and allowing each dressing to remain for three days; the skinning went on rapidly and without any intermission. A curious point about the grafts was the fact that they retained their vitality for weeks, but still showed no appearance of enlarging or shooting out skin; they could be seen until the skinning from the edges came up to them.

Gentlemen, you all know how difficult it is in large ulcers to get the surfaces to skin over with the usual wet method of dressing. Grafting of skin has been resorted to with success in many such cases, but it is rather a troublesome and tedious process, and painful to a generous friend who may be willing to part with some of his skin, or to the patient if there is no substitute. I have no hesitation or doubt in saying that this *dry dressing*, when properly carried out with due care, will do away with the necessity of grafting.

All ulcers, from the healthy to the indolent, are less or more weak. It has been customary in treating ulcers to have the lint cut as near the size of the sore as possible, in wet dressing; the larger the piece of lint used the more apt we are to weaken the part and retard the healing. This made us resort to a stimulating dressing in a variety of ways, the advantage of this *dry dressing* being that it is stimulating from the very outset. There is enough moisture retained in the three or four folds of muslin to protect the raw surface until a sufficiency of pus is thrown out which in turn protects it. The moisture that is derived from the surrounding soft parts embraced within the Mackintosh covering is all that is necessary, more than that would be a source of weakness. The antiseptic action of the boracic acid lint keeps the parts perfectly fresh and sweet for the three days the dressings remain, large as the surface may be, and perhaps longer if the patient be able to take particular care of himself and prevent the dressings becoming loosened.

The oxide of zinc ointment I consider only useful when the raw surface is very large, as in that case the patient bears the removal of the dressings with greater ease and comfort, which is of the greatest importance to a young or irritable patient. The dry boracic acid lint, but for that, can be used at the very beginning.

As to this little fellow, you see that both his legs are as straight as legs can be, with no difference in length; the joint of the left knee acts perfectly well, and the anterior surface of the thigh was completely skinned over by the first of this month.

In comparing these two cases—the lady's age being between 60 and 70 and the boy's about 7—the latter had the advantage of youth and a good constitution, the former had not. No improvement took place in her case while I used the wet dressing, but immediately I began the dry, healing took place. In regard to the grafting in the case of the boy, which was not tried in the other patient, no benefit I believe was derived from it.

The strength of the boracic acid lotion used was an ounce and a half to forty of water, first dissolved in several ounces of boiling water and put into a bottle that would take forty fluid ounces, and when required the whole shaken up, and the quantity to be used at dressing time heated, being more than a saturated solution.

CURRENT TOPICS.

Treatment of Floating Kidney by Fixation.—We are informed that Dr. David Newman has performed, for the first time in this country, the operation of nephroraphy. The right kidney was exposed by a vertical incision in the loin, immediately external to the outer edge of the quadratus lumborum, and extending from the lowermost rib to the crest of the ilium. The capsule containing the kidney was then opened and stitched to the edges of the wound, and two catgut sutures were passed through the cortex of the kidney, the muscles, fascia, and skin, and secured externally by means of buttons. The operation was performed three weeks ago; the patient has now completely recovered from the effects of it, and the severe symptoms from which she previously suffered have entirely disappeared. Dr. Newman described the operation at the last meeting of the Pathological and Clinical Society, and will no doubt publish the case when the ultimate results of the operation are known.

Trendelenburg's Method of Amputation at the Hip Joint.—In the American *Medical Journal* Dr. Varick of

Jersey City Hospital describes an amputation at the hip joint, which was successful mainly through the saving of blood by using Prof. Trendelenburg's method of preventing hæmorrhage. This method requires a flat steel rod a foot long and ¼ inch wide, with a movable lance-shaped point, the rod to be biconvex in section, ¼ of an inch thick in the middle, with blunt but smooth edges. This rod is thrust obliquely through the soft parts in front of the joint, in the same way as the two-edged knife in the well known method of Lisfranc, but nearly an inch higher. The rod enters 1½ in. below the anterior superior spinous process of the ilium, passes between the femoral artery and the bone, and emerges at the fold of the scrotum. The point being removed, an elastic band is firmly wound figure-of-8 fashion around the projecting ends of the rod, compressing effectually the great vessels. Lisfranc's knife is then introduced a little below the rod, and by cutting from within outwards in the usual way the anterior flap is formed. The vessels being tied, the band and rod are removed and the joint disarticulated and the posterior flap formed. The patient made a good recovery.—*Pacific Med. and Surg. Journal.*

[In *The American Journal of the Medical Sciences*, vol. lxxxiii, p. 582, there will be found an almost exact copy of an item which appeared in our pages in February, 1882, in which the operation performed by Trendelenburg is described. In the same journal for October, 1882, Dr. Varick describes the operation performed by him, and in his paper transcribes the description from the former number of the *American Journal.* Dr. Varick does not appear to have seen Trendelenburg's original paper in the *Archiv für Klin. Chirg.*, Bd. 26, H. 4, otherwise he would have known that Professor Trendelenburg does not claim the operation as his own, but acknowledges having received the idea of the operation, which he, however, modified slightly, from an article by Dr. David Newman, of Glasgow, which appeared in our pages in October, 1876.]

To the Editors of the "Glasgow Medical Journal."

2 Park Terrace, Queen's Drive,
Crosshill, *16th April, 1883.*

Dear Sirs,—In your last issue appears a letter from Mr. Cadell, making correction of a statement contained in my paper on the "Treatment of Syphilis." The words repudiated by him do not appear in inverted commas in my paper, and therefore did not purport to be *ipsissima verba*, but to convey the general effect of his teaching. Nevertheless, I frankly regret that he should have found it necessary to take exception to my manner of putting the case, and acknowledge that a re-perusal of his paper justifies his contention that he did not put forth his views as those of the majority.

At the same time, I regret having to remove a misconception which Mr. Cadell himself has made. I do not think the language of my paper can be fairly interpreted to mean that I believe "mercury to be necessary to the healing of hard chancre" *in all cases*. I certainly did not mean to convey that idea, and none of the members of the Southern Medical Society (to whom the paper was originally read) derived that idea from it. What I contend is, that in the *great majority* of cases of typical hard chancre, the tendency is not towards spontaneous repair, but that it becomes the reverse whenever the patient is placed upon mercury. I believe this to be the rule, but *I know* there are exceptions to it, as there are to every rule. I have a patient presently under treatment who appears to have a hard chancre (according to him it did not appear till six weeks after connection, but by reason of its situation at the frænum, its typical features are somewhat modified), which is healing without mercury. But, at the same time, I have another whose sore was not healed at the end of nine months when he first came to me, cachectic, impotent, and covered with secondary eruptions; the sore yielded at once, and healed before the patient had taken 36 Plummer's pills, with a local dressing of iodoform vaseline.

I must also ask to be allowed to remark that I do not think the main question at issue, to wit, Whether syphilis can be cured without mercury? is one upon which we can agree to differ, as it is not a question of *opinion*, but a most important question of *fact*. If a patient suffering from syphilis can be cured, say within three years, without the use of mercury, then the fact ought to be demonstrated.

I am, dear Sirs, yours faithfully,

ROBT. PARK.

REVIEWS.

Anatomy, Descriptive and Surgical. By HENRY GRAY, F.R.S., with an Introduction on General Anatomy and Development. By T. HOLMES, M.A.Cantab. Tenth Edition. Edited by T. PICKERING PICK, Surgeon to St. George's Hospital. London: Longmans, Green & Co.

GRAY'S *Anatomy* has been long a favourite, and the appearance of a tenth edition, carefully revised, published by the same enterprising firm that gave to the world the other day a ninth edition of Quain's *Anatomy,* is proof enough that it continues to hold its ground, and that the publishers look on it as a valuable property worth conserving. We think that it deserves to hold its place, though even this new edition is far from perfect. For we must judge by comparison with other textbooks in the field; and truly we know not any work in the English language, nor any work at all, to which we can point, and tell the student that it is all that we could desire.

A textbook on anatomy should be concise, scientific, and complete; details should be stated with clear precision, and their bearing on morphological generalisation, on function, and on surgical or medical practice, should be indicated shortly in such a manner as to awaken the intelligent interest of the student, and help him onwards by fostering the conviction that the dead body can be made to live in the mind which earnestly studies it. For such a work we wait, and shall wait long.

Gray's *Anatomy* has the advantage of being in one volume, though the page is large, the type sufficiently small, and the thickness greater than we believe to be necessary for the scope. In fact, it is beyond all question exceedingly verbose. If we compare with the ninth edition of Quain, to which we have already referred, we shall find that the voluminous character of the latter depends mainly on a different cause—namely, the introduction of many things not even alluded to in such a work as Gray, not to mention an occasional unwise expatiation on trifles not necessary to the completeness of even a work that aims, as Quain's *Anatomy* does, at giving a full account of the state of the science at the present time. It is a delicate problem which presents itself to the intending purchaser who happens to turn up the second page of the advertisements bound up in the volume before us. There he finds the volume itself, or rather the previous edition, advertised, and immediately above it a two volume book at a slightly higher figure; and a cursory inspection of the works themselves will show him that the two volume book professes to go a great deal deeper into the subject than the book in one volume; but that each is supposed to be fitted for its purpose, whatever that may be, and not considered by the publishers as likely to destroy the sale of the other.

Gray's *Anatomy* owes much of its popularity to its illustrations. They have been much found fault with as inartistic; but they have the advantage of being of large size, and students like them, and find them convenient. It is a serious fault, however, that the inartistic passes in many instances into the inaccurate. Thus, in fig. 363, the ascending colon is, at the lower part of the figure, represented as coming up from an unknown region and crossing the external iliac artery parallel to the ureter, while the spleen is shaped like an ear. Fig. 269 is said to represent the convolutions and fissures of the outer surface of the cerebral hemisphere; but, however well it may be adapted to enable a student to grind his way through an examination, it is such a diagram as Nature refuses to accommodate herself to in any circumstances of race or development; while the view of the island of Reil, in the following figure, is equally misleading. Many of the original representations of the bones we count as extremely good.

Reference is made very properly in a distinct footnote to the different view now taken of the position of the stomach from that which was formerly current, according to which its long diameter was held to be horizontal. But we cannot admit that the corrected view of the matter is so recent as the International Congress in London, as the note seems to indicate. It was pointed out by Luschka, in his *Anat. des Menschen*, twenty years ago, as is mentioned by Prof. Turner, in his *Introduction to Human Anatomy*, in which also he figures the stomach in the vertical position. For ourselves, we have never failed for many years past to observe that when the stomach is empty its lesser curve is vertical in at least two-thirds of its extent. In the account of the mucous membrane of the stomach a serious error occurs, which does not look well for the author's personal acquaintance with the things which he describes. The description of the gastric glands is an account not of what is found in the human stomach, but of the glands of the dog's stomach, and the same remark holds good of the woodcuts showing the peptic and mucous glands; yet no intimation is given that it is not the human structure which is figured and described. We attract attention the more to this, because teachers are often very careless in allowing students to go away with the idea that the structure of the mucous membrane in the dog's stomach and in the human stomach is much the same, although no one could fall into such an error who had examined the microscopy of the human stomach for himself. Generally, we feel, we confess, a certain sense of unreality in reading the parts devoted to the description of microscopic structure, and do not consider them as at all so good as the surgical anatomy and some other parts.

The description of the membranous labyrinth is illustrated very properly with a diagram, such as has made its appearance in more than one other book, in which the scala media or canalis cochleæ is introduced so as to exhibit that it is continuous through the canalis reuniens with the saccule, and is the only part of the cochlea belonging to the membranous labyrinth; but nowhere is this clearly brought out in the text. On the contrary, the whole cochlea is described in the same section with the osseous labyrinth, and a cochlear part of the membranous labyrinth is not alluded to in the section headed "the membranous labyrinth;" so that few students, if any, will be able to gather out of the text the only truth which the diagram is fitted to illustrate. In the description of the eye a diagram is continued in this edition in which the iris is represented as hanging free in the aqueous humour at a little distance in front of the lens. This surely ought to have been corrected, seeing that the iris had been already discovered to be resting on the lens ere yet the first edition of Gray's *Anatomy* had seen the light.

Another subject on which we cannot forbear to comment is the development of the teeth. The account is not such as to give the student any clear idea of the process; and the writer has spread the delusion originated, so far as we can trace it, by Kölliker, that the observations of Arnold and Goodsir are at variance with later research. The truth is simply that neither Arnold nor Goodsir pretended to give an account of the disposition of the epithelium. They worked with specimens from which the epithelium was brushed away, and accurately described what was in these circumstances brought to view; and it should be remembered that they wrote many years before the preparation of hardened sections for the microscope had been introduced.

But though we have thought it necessary to illustrate some of the defects of this work, we wish to record distinctly that there is much in it to admire, and that we believe the present edition will continue to hold the place in the esteem of students which has been held by its predecessors.

A Guide to the Medical Profession: A comprehensive Manual conveying the means of entering the Medical Profession in the chief countries of the world. By Edwin Wooton, Edited and with Preface by Lyttleton Forbes Winslow, M.B., D.C.L. London: L. Upcott Gill. 1883.

"I have been asked to peruse the following pages and to write this short preface, and have done so with much pleasure, being confident that, ere long, the book will be in the hands of all professional men, and in the library of every medical school." Such is Dr. Winslow's explanation of his connection with the book, although the title page asserts that it is "edited" by him; surely in making such a statement he stands self-condemned. The compiler, not being a medical man, desired to have the prestige of a well known medical name to assist the sale, and Dr. Winslow seems to have allowed his name to be used with this consideration (whatever other may have been given), that he would thus be advertised to the medical profession and to medical students generally as "M.B., D.C.L., author of a Manual on Lunacy," &c., &c. Had the book been a valuable one, and the information it contains accurate, we might have been prepared to overlook this peculiarity in the mode of its production; for we should then have concluded that Dr. Winslow's supervision had been greater than he claims, and that the presence of a medical editor had ensured a more fair representation of medical affairs than a layman could be expected to attain to. But a careful perusal of it fails to satisfy us in this particular. Here, for instance, is a sentence which is not only a gem of English composition, but contains a statement somewhat startling to the benighted alumni of our University:—"Glasgow—Candidates for the Doctorate must be 24 years of age, and have held the Bachelorship for two years. This is so for a residential university as to enforce one year's personal attendance; one other such year must be passed at some recognised university." Those among us who have striven so hard to maintain the reputation of the Faculty of Physicians and Surgeons, and who deny that there is any "competition downwards" practised in Glasgow, will find with dismay, if they trust to this veracious manual, that the license of the Faculty can be obtained for £9, 9s., namely, £6, 6s. for the first half, and £3, 3s. for the final.

If it were not for the almost absolute certainty that the work will never reach a second edition, it might be worth while paying Mr. Wooton to come to Scotland, if only to give him the opportunity of finding out that there are other medical schools in this country besides Edinburgh University, he might thus correct the errors and omissions of the present production. As indicating the low estimate in which we are held by this ignorant layman, we may point out that, while the account of the medical schools in London extends to forty-three pages, that of the English provinces to twenty-two pages, and that of the Irish schools to eleven pages, the Scotch schools are dismissed in two and a half pages, being exactly the same amount of space as is devoted to the "medical education of women." Nor is the information given as to the regulations of universities and schools distinguished by such accuracy as might compensate for its scantiness, for we are told that there are three examinations (instead of four) for the M.B. of Glasgow University; and in the regulations of Edinburgh University the same mistake occurs, but in this case the confusion is even greater, for while in one place we are told there are four examinations, in the list of fees (p. 85) they figure as three, and on p. 89 they are spoken of as two and three, not only on the same page, but in succeeding sentences. Again, in the regulations of the Glasgow Dental School, we are told that the fees for anatomy, anatomy of head and neck, and dissection are £7, 7s. *each;* no doubt the lecturer on Anatomy in Anderson's College wishes it were so.

The account given of the American, Colonial, and Foreign Schools is interesting; but, if not more accurate than that of the British Schools, is not worthy of much confidence. The United States appears to be more than sufficiently supplied with doctors, for, according to Professor Alfred Mercer, they average 1 to every 600 inhabitants, or to put it in another form, there are 2 doctors in the United States to 1 in Canada, nearly 3 to 1 in Great Britain, more than 4 to 1 in France, and 5 to 1 in Germany. This is explained "by the cheapness of American diplomas, and the few legal restrictions on the practice of medicine with or without a diploma or any known qualification whatever," and possibly also by the fact here brought out that, out of the eighty-six licensing bodies, there are only five which require "a three years' graded course of nine months in each year, with annual examinations on the studies of the year, and with fair preliminary examination." So much for free trade in medical and surgical diplomas.

A work of this nature, if well put together, accurate as to details, and reasonably brought up to date, would be both interesting and useful, and we regret therefore that we cannot commend the present book to the attention of the readers of the *Journal.*

A Treatise on the Theory and Practice of Medicine. By JOHN SYER BRISTOWE, M.D.,Lond., F.R.S. Fourth Edition. London: Smith, Elder & Co. 1882.

THE fact that, in the comparatively short space of six years, no less than four editions of Dr. Bristowe's book have been called for, is proof sufficient of the very high place it has taken among works on the practice of medicine. There are few books, with which we are acquainted, that have been more successful, and, perhaps we may be permitted to add, that have been more deservedly so. After the two somewhat exhaustive notices of the work that have already appeared in our pages, it is unnecessary at present to discuss it in detail. We would only add, however, that the high standard of the volume is still maintained, and that several additions and alterations have been made in the present edition, which bring it up to the present date. The following quotation from the preface will serve to show what these are:—"The more important corrections in the text are comprised in the chapters on Myxœdema, Parasitic Affections, Diabetes, Reflex Action, Lateral Sclerosis, and the legal management of Lunatics.

"The chief literary additions relate to Stammering, Ophthalmoplegia, Acute Ascending Spinal Paralysis, the mitigation or attenuation of Contagia, and Koch's recent discovery of Bacilli in Tubercle.

"The new woodcuts are about forty in number, of which nearly all illustrate the first part of the volume, or that devoted to general pathology. These, like former woodcuts, are mainly from original drawings of my own."

It will thus be seen that the student has at his disposal a work of the very highest character, in which all the most recent theories and views will be found; and we can most heartily recommend it to all who are interested in the scientific study of the practice of medicine.

A Supplementary Catalogue of the Pathological Museum of St. George's Hospital. By ISAMBARD OWEN, M.D., Curator. London: J. & A. Churchill. 1882.

IN this work a concise and interesting account of all the specimens which have been added to the museum of St. George's Hospital, between the years 1866-1881, is given. In all, during this period, 881 new preparations have been added, making up a total number of 4,284; and if one can judge of the first descriptive catalogue from the supplementary one now in our hands, the entire work must form a very valuable contribution to the literature of pathological anatomy. Of course such a work is chiefly interesting to the professed pathologist; but, to all who are interested in recording the rarer cases met with in public and private practice, the volume will be of great value as a book of reference. The mode of classification is simple and practical, and will be readily understood by any one referring to the book.

We congratulate the author upon the excellence of his work, which must have involved the expenditure of a very considerable amount of time and trouble.

St. Thomas's Hospital Reports. New Series. Edited by DR. ROBERT CORY and MR. FRANCIS MASON. Vol. XI. London: J. & A. Churchill. 1882.

IN the present volume of Reports, a highly interesting series of papers on various subjects is brought together, which illustrates very well the high character of the work carried on at St. Thomas's Hospital. In the list of contributors to the volume the following well known names are found—Dr. Thomas B. Peacock (Disease of the Aortic Valves, probably originating in malformation); Dr. Wm. M. Ord (On some cases of Paroxysmal Pyrexia simulating Ague); Dr. John Harley (Fæcal Retention, especially as it affects the cæcum); and Dr. J. S. Bristowe (On Hydatid Tumours of the Abdomen and Tumours simulating them).

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR BUCHANAN'S WARDS.

CASE OF PERIOSTITIC ABSCESS OF LOWER JAW, WITH SPONTANEOUS HÆMORRHAGE—LIGATURE OF COMMON CAROTID—DEATH.—James W., æt. 53, baker, was admitted 13th January, 1883, on account of a sinus on the right side of the neck, leading down to a cavity surrounding the angle of the jaw, which was denuded of periosteum. There was also an opening communicating with the mouth, and from both a very foul discharge escaped. In the month of August last, on account of severe toothache, the backmost tooth on the right side of the lower jaw was extracted by a dentist. This was not, however, attended by any relief to the symptoms, as the pain continued very severe, and soon afterwards the right side of his face began to swell. Fomentations were applied without benefit, and at length an abscess formed. This was opened first from the mouth, and latterly externally, and on both occasions a large quantity of pus was evacuated. Neither sinus healed, and, as the condition was getting gradually worse, he came into hospital. On admission the abscess cavity was thoroughly cleaned out, a drainage tube inserted, and fomentations applied.

15th January.—About 1 P.M., Mr. Goff, house surgeon, was suddenly called to see him, as he was bleeding profusely from the neck. On examination it was found that the bleeding was from one of the branches of the external carotid, but the relationship of parts was so much obscured by the swelling that it was quite impossible to determine which vessel was involved. The external wound was enlarged, but, as the bleeding point could not be reached, the cavity was firmly plugged, and Dr. Buchanan was sent for. On his arrival the cavity was freely opened in all directions, but as it was quite impossible in the sloughy state of the surrounding tissues to discover any bleeding point, the common carotid artery was ligatured in its continuity. Soon after recovering from the effects of the chloroform the patient expressed himself as feeling well and comfortable. In the evening, except that the affected side of the head was considerably colder than the other, nothing noteworthy was remarked.

17th February.—The ligature of the carotid has produced very little disturbance, and has been well recovered from; but there is no improvement in the local condition, and the patient has been gradually losing flesh and becoming weaker. There has been all along, both from the external wound and into the mouth, a profuse discharge of very putrid pus—one of his greatest troubles being that he is forced to swallow some of this when taking his food.

19th February.—This patient is perceptibly getting weaker, and now, although latterly he has been taking nothing but brandy and milk, he can't even manage this, as it is almost impossible for him to open his mouth. For the same reason, too, it was found impossible to pass a tube into the stomach. His breath and the discharges from the wound have a very offensive odour.

22nd February.—At 1·45 A.M. this morning, he died from exhaustion, due apparently in great part to his inability to take food, and to his swallowing the putrid discharge.

A *post-mortem* examination was made by Dr. Coats, of which the following is a note:—The body is emaciated. There is an elongated wound extending from just behind the lower extremity of the ear obliquely, downwards and forwards, to about an inch from the middle line, and measuring altogether about 4 inches. The wound is united for about ¾ of an inch in the lower part of its course, but elsewhere it communicates with a ragged suppurating cavity. Nothing definite can be made out as to the state of the vessels in the wound, owing to the decomposition and sloughy condition of the parts. The heart presents nothing remarkable. The left lung is non-adherent. At the extreme lower part of the upper lobe there is an abscess, about the size of a hazel nut, filled with a creamy pus. Otherwise this lung is normal in appearance. The right lung is somewhat adherent. Towards the base there are several softened yellowish areas having the characters of gangrene of the lung. Otherwise the organ is generally œdematous. The spleen is normal; the kidneys are normal in size, and although the capsules are somewhat adherent, their tissue is not otherwise remarkable. The liver presents well marked fatty infiltration, and weighs 47 ounces. There is considerable œdema of the membranes of the brain, and the following is the condition of the circle of Willis and arteries at the base. The right vertebral artery is of very insignificant size as compared with the left—not more than ⅛th of its size. The posterior communicating artery of the right side is not more than a third of the size of the left. The internal carotid, where cut across in the brain, is of equal size on either side, as are also the anterior and middle cerebrals on each side. The anterior communicating artery is not larger than normal.

FROM DR. CAMERON'S WARDS.

MALIGNANT ŒSOPHAGEAL STRICTURE, WITH GANGRENE OF THE LUNG. [Reported by Norman M. Maclehose, M.B. and C.M., House Surgeon.]—Mrs. K., æt. 50, admitted to Ward XVII, on 21st February, 1883, complaining of pain and difficulty in swallowing. Patient is very much emaciated, and says that her health, which was previously good, has failed very rapidly since the onset of her present illness. She was quite well till October of last year, when, for the first time, she began to suffer from the symptoms referred to above, and these were especially severe when any attempt to take solid food was made. Vomiting, which sometimes occurred after meals, relieved the pain considerably, and she also found that repeated draughts of cold water seemed to aid the passage of the food into the stomach. On no occasion, so far as she is aware, was there any blood in the vomited matter. The pain is situated in the epigastrium, and she describes it by saying that she feels as if a heavy weight were pressing on a painful spot. In December last she became so much worse that she sought medical aid, and since then œsophageal bougies have been passed at intervals in order that she might be able to take liquid food. Pulse is normal, and about 70. Tongue moist and furred in the centre. The urine is natural; and the bowels confined. The family history is unimportant. Patient was on the medical side of the house for about ten days before her admission to Ward XVII. While there she had a slight attack of pleurisy in the left side which was relieved on the application of a fly blister.

1st March.—Bougies are being passed every fifth or sixth day. The stricture is situated low down in the œsophagus, apparently close to the cardiac end of the stomach, and resists the passage of the instrument which, however, with a little gentle pressure slips in suddenly with almost a jerking motion.

10th March.—Had another attack of pleurisy in the left side. This is now passing off. Fly blisters were applied.

18th March.—Patient is evidently in a state of gradually decreasing strength. The food, which is entirely liquid, is swallowed with more difficulty than before, and the stricture is evidently narrowing in its calibre, as evidenced by the increased difficulty in passing instruments. The question of performing gastrostomy was considered, but was decided against, as the woman is far gone, and the tumour evidently malignant in its character. The repeated pleurisy also was thought a very unfavourable symptom. Patient is being regularly fed by enemata, and encouraged to take as much as possible by the mouth.

25th March.—Pleuritic friction is again noticed in the same locality as before. There is also a good deal of expectoration which has a very fœtid odour, resembling, though in a minor degree, that noticed in cases of gangrene of the lung. The woman is sinking fast and emaciation is extreme.

6th April.—Patient died to-day. Till the last she was able to take liquid food in small quantity by the mouth. During the past few days she had a very severe cough, and the accompanying expectoration was profuse and exceedingly fœtid.

The following is the report of the *post-mortem* examination, which was made by Mr. J. Lindsay Steven, M.B.

External Appearances.—There is very great emaciation of the body.

Chest.—The pericardium contains a small quantity of straw-coloured fluid, and the visceral layer is slightly œdematous, having an appearance as if little masses of gelatine were projecting from the heart wall. There is no fat on its surface, but otherwise it presents normal characters. The trachea, lungs, œsophagus, stomach, and liver, are removed together. On cutting up the œsophagus a tumour about the size of a Brazil nut is discovered just before the stomach is reached. On introducing the little finger the calibre of the tube is found to be very considerably narrowed by the tumour, but a large probe passes with ease. The tumour is very hard, about two inches in its long diameter, and strictly confined to the wall of the gullet, there being no matting of, or adhesion to, surrounding parts. On laying open this part of the tube it is seen to be about one-fourth of an inch in thickness, and in it three distinct layers, corresponding to the coats of the gullet, can be seen. There is slight ulceration of some parts of the mucous surface, but as a rule this is absent. There is distinct thickening of the muscular layer at the site of the tumour, and this is continued for some distance upwards. The growth is sharply defined from surrounding tissues, but its outline is very sinuous. One of the small glands in the neighbourhood is enlarged. The stomach itself and the liver are healthy. The right lung is non-adherent, but very hyperæmic throughout. The left lung is slightly adherent over its lower lobe, and on breaking the adhesions, which are very soft, the lung tissue breaks down. Corresponding to the adherent surface a large slough of the pleura has occurred, and, on removing the organ, the greater part of the necrosed pleura is left sticking to the chest wall. Beneath this area a large cavity is found filled with dirty greenish fluid, and broken down lung tissue, and presenting all the characters of a gangrene. No apparent cause of this condition was discoverable. Other organs healthy. Upon microscopic examination, the tumour is found to consist of masses of large cells lying in a stroma which in some situations is more abundant than in others. In some places two masses of leucocytes are seen.

The groups are seen to penetrate the muscular layer of the œsophagus, thus involving also the external coats in which masses similar to those noted in the mucous coat are seen. The external coat is greatly thickened, but this is in great part due to increased development of connective tissue. The enlarged gland is filled with creamy material, in which are observed numerous highly fatty cancerous cells. No secondary deposits are discovered in the lungs, but extensive catarrhal pneumonia exists around the gangrenous portion. The tumour presents all the characters of a scirrhus, which is somewhat rare in this situation.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

The Tubercle Bacillus.—The following abstract of observations is taken partly from a *résumé* to be found in the *Cbl. f. d. Med. Wiss.*, No. 12, 1883, and partly from the original sources.

1. Hiller (*Zschr. f. Klin. Med.*, v, p. 638) examined the sputum in six cases of hæmoptysis. In three of these, which the whole circumstances justified him in regarding as cases of *initial* hæmoptysis, the tubercle bacillus was found. As its presence was not only proved microscopically, but inoculations on guinea-pigs were successful, Hiller considers he has shown that initial hæmoptysis is a sign, and one of the results, of pre-existing infection of the lungs.

2. Dr. P. Guttmann (*Berl. Klin. Wochenschrift*, 1882, No. 52) condemns Baumgarten's "easy method" of demonstrating the tubercle bacillus (see this *Journal*, Sept., 1882); describes a method (Ehrlich's) by which four specimens of sputum may be prepared in about half an hour; uses undiluted officinal nitric acid for decolorising the sputum. In 100 preparations made from phthisical sputum, the bacillus was found in only about one-fourth of the whole, sometimes sparingly, sometimes in great abundance. Great variations in number are met with, the field (powers, 300 to 400) being sometimes crowded with bacilli, at others showing but few. Even from the same mass of sputum some preparations show very numerous bacilli, others none. As a proof of the superiority of this test for phthisis over the old test—viz., the search for elastic

fibres, Dr. G. states, that of the above mentioned 100 preparations, in which about a fourth part showed the bacillus, the elastic fibres were found in only three.

3. Balmer and Fräntzel (*Berl. Klin. Wochenschr.*, No. 45, 1882) examined the sputum in 120 cases of phthisis, and detected the tubercle bacillus in *all*. In the sputum of other patients suffering from non-phthisical lung diseases, the bacillus was not found. They followed Ehrlich's method of staining, except that the dye (gentian-violet or fuchsin) was dissolved directly in the anilin-water, one part of the colouring matter to fifty of the solvent. They insist that the staining solution shall be freshly prepared and filtered, and that the preparations lie in it for twenty-four hours; the bacilli are thus stained so deeply that they catch the eye even if few in number. They admit the complete absence of bacilli only when in four to six different days the preparations are free of them. They found that the prognosis was grave in direct proportion to the number and development of the bacilli present. In any one case also the number of bacilli is not constant, increasing as the destructive process advances. In cases which progress rapidly the bacilli are large, and exhibit spores. Fever is always high, with numerous bacilli; when fever is absent or diminished the spores are few and slightly developed. These abortive forms were also found in old closed cavities. In the contents of recent cavities, and in the sputum from these, the bacilli were particularly abundant; in the walls of such cavities, on the other hand, they were very few in number. From this observation is drawn the inference that the cavity-contents furnish a better soil for the development of the bacilli than the living lung tissue of the cavity-wall. The bacillus was found in abundance also in the tissue and secretion of tuberculous ulcers of the tongue, in the walls of intestinal ulcers, and in the pus from a tuberculous knee-joint; the last observation, which has been confirmed by others, shows that the abundance of the bacillus in pulmonary cavities is not due to the presence of oxygen. Bacilli were demonstrated in the sputum even after it had been completely dissolved in potash solution, and also after treatment for twenty-four hours with a 0·1 per cent solution of corrosive sublimate.

4. Lichtheim (*Fortschitte der Med.*, No. 1, 1883) finds that he can rely with some confidence on the bacillus test for tuberculosis. He mentions a case in which the presence of the bacillus dispelled the notion that the disease he had to deal with was a syphilitic lung affection, and in another he demonstrated the tuberculous character of an acute pneumonic affection at a very early stage; the absence of the bacillus in

perforating empyema and in bronchiectasis is also to be borne in mind. Of all the distinctly phthisical cases Lichtheim has examined, the bacillus was wanting in only two; in one of these the disease was of old standing and was apparently in process of cure, and in the other only one examination was made. He believes that the bacillus is occasionally found even in cases in which no physical signs are discoverable, and quotes an example of this from his own experience. In some cases, however, he has failed to find the bacillus even when such symptoms were present as troublesome cough, difficult expectoration, scanty sputum of a mucous character and with few particles in it; here the sputum evidently came mostly from the upper part of the air passages, but even the opaque portions gave a negative result. He concludes that most of these patients were not tuberculous; but admits frankly that, as they all occurred in private practice, the sputum was not so often examined as in hospital cases. Lichtheim then holds (1) that bacilli are invariably to be found in the abundant purulent sputum of the phthisical; (2) that their presence is connected with tuberculous destructive processes in the lung, in direct communication with the air passages; (3) and that the bacilli are wanting, even in tuberculous cases, if the last mentioned condition, that is, direct communication with the air, fails. He rejects Balmer and Fräntzel's view that the gravity of the prognosis is invariably proportionate to the number of bacilli found; apart from cases in which the bacilli are present in multitudes in each field, in which the prognosis is always grave, there occur many cases slow in their progress and associated with abundance of bacilli in the sputum, and others rapid in their progress but with comparatively few bacilli in the sputum. He suggests that occasionally abundance of bacilli may be due to the length of time the sputum is detained in the lungs. Balmer and Fräntzel quote the observation that bacilli were detected in sputum dissolved in potash solution, and also in sputum steeped in sublimate solution, as indicating that disinfection of tuberculous materials is a matter of difficulty; Lichtheim dissents from this view, and observes that staining is no proof that the bacilli retain their activity. The superheated anthrax bacillus takes the stain readily, though its power of development is destroyed at comparatively low temperatures. Further, it is probably not the bacilli but their unstainable spores which offer difficulties in the way of disinfection. Lichtheim stains with Weigert's modification of Ehrlich's solution, immersing the preparation only about a quarter of an hour; Balmer and Fräntzel's immersion of twenty-four hours was necessary only because they used weaker solutions. The time may be shortened if, as Rindfleisch suggests, the staining be carried on over the spirit lamp. Mounted in balsam, or in the chloroform solution of balsam, the bacilli soon lose their stain; he finds that in balsam, thinned with oil of cloves, they retain their colour at least half a year. Lichtheim makes the important statement that he has found the bacillus in the diarrhœal stools in tuberculous disease of the bowel. May this not be due to the swallowing of sputum containing the bacilli? Lichtheim has convinced himself that this is not the case. The bacilli are much fewer in the stools than in the sputum; and as the stools consist largely of other micro-organisms, simple staining is better than double staining, the coloured tubercle bacilli being then easily seen among the other colourless microbes. In this connection Lichtheim makes another important observation: that in many stools, tuberculous and non-tuberculous, he has found, but always sparingly, a tolerably large round micrococcus, sometimes isolated, at other times arranged in groups, which after staining is also not decolorised in nitric acid.

5. As the result of the examination of a large number of consumptive cases in various stages of the disease, Dr. A. Pfeiffer (*Berl. Klin. Woch.*, No. 3, 1883), is persuaded that as the disease advances the number of the bacilli present increases. Occasionally, when the bacilli reappeared in the sputum after having been absent for three or four days, they seemed smaller and contained no spores. Pfeiffer employs Ehrlich's method of examination, but to facilitate the detection of the bacilli he adopts Long's suggestion to place the whole mass of sputum in an alkaline solution; in the greyish-green compact streaks of sputum are then seen small glancing white particles, which consist almost entirely of colonies of tubercle bacilli.

6. Dr. H. F. Formad, Lecturer on Experimental Pathology in the University of Pennsylvania, has been conducting an elaborate series of researches on this subject, from which he draws the following deductions. We give them as a serious American criticism on Koch's doctrines. (1) The predisposition to tuberculosis in some men and animals, the so-called scrofulous habit, lies in the anatomy of the connective tissue of the individual, the peculiarity being a narrowness of the lymph spaces, and their partial obliteration by cellular elements. (2) Only beings with such anomalous structure of connective tissue can have primary tuberculosis, and such animals invariably do become tuberculous from any injury resulting in inflammation, or from repeated injuries. (3) Scrofulous beings can have no other than a tuberculous inflammation, although it may remain local and harmless. (4) Non-scrofulous men or

animals may acquire the predisposition to tuberculosis through malnutrition and confinement, the latter bringing on the above mentioned anatomical peculiarities in the connective tissue. (5) No external ætiological influences are necessary to cause tuberculous disease other than those which ordinarily produce inflammation, and even scrofulous beings will not become tuberculous unless local inflammation is set up. No inflammation, no tuberculosis. (6) Non-scrofulous animals, so far as can be established now, may acquire tuberculous disease through injuries of serous membranes—viz., peritoneum, pleura, &c., and even then without any special virus whatsoever. Clinical observations on the *post-mortem* table show similar conditions and prove the same in man. (Dr. Formad also claims that Koch's own experiments are really in favour of this proposition; but that he has overlooked the inference.) (7) The bacilli, which it is the merit of Koch to have first proved to infest tissues affected by tuberculous disease, are not necessary for its causation, even if a special organism exist and be really possessed of such property. The presence of bacilli (so far as our present knowledge goes) is secondary, and appears to *condition* the complete destruction of the tissue already diseased and infested by them, and this destruction is in direct proportion to the quantity of the organisms, which thus regulate the prognosis. The tuberculous tissue seems to serve merely as a nidus for the growth of the bacillus. (8) From the results of microscopic examination, from numerous observations upon the *post-mortem* table, and on clinical grounds, he has come to the conclusion that phthisis is not a specific infectious disease, but that the individuals suffering from tuberculous disease are specific themselves originally, and form a special species of mankind—the "scrofulous." (9) Scrofulosis is a condition which may arise from malnutrition and seclusion in any being, and thus may be produced artificially. It always depends upon the demonstrated anatomical changes in the connective tissue. (10) An analysis of Koch's experiments shows that he has not proved the parasitic nature of phthisis, or that there exists a special *bacillus tuberculosis;* so that the infectiousness of tuberculous disease is still *sub judice. Philadelphia Medical Times.* 18th November, 1882.

7. At the Royal Society, 8th November, a "Note on the Discovery of Bacilli in the Condensed Aqueous Vapour of the Breath of Persons affected with Phthisis," was read by Dr. Arthur Ransome, communicated by Dr. W. Roberts, F.R.S. The author caused several patients suffering from advanced phthisis to breathe into glass globules surrounded by ice and salt; the aqueous vapour of the breath, in condensing, was

found to carry down with it all the organic matter borne by the expired air. In order to carry down the organic matter, and to afford a basis to attach the material to the microscopic cover-glasses, fresh white of egg, or a little mucus, free from bacilli, was added to the fluid. No attempt was made to sterilise the fluids, as the ordinary bacteria of putrefaction are not stained by the process used. In the aqueous vapour obtained from two of the cases, specimens of bacillus were found which took the staining in the same manner as the bacillus found in phthisical sputa and in tubercle. The organism was not found in several other cases, nor yet in the aqueous vapour condensed in the waiting room of the Manchester Consumption Hospital.—*Brit. Med. Journ.* 16th December, 1882.

8. Dr. F. Ziehl's researches (*Deutsche Med. Woch.*, No. 5, 1883), here quoted, concerned 73 cases of clinically unquestionable tuberculosis, and 34 cases of a different character; in the former the bacillus was always found, in the latter not. The author's method of examination, published in *Deutsche Med. Woch.*, No. 33, 1882, is recommended; it consists in first staining the preparations with fuchsin, and then immersing them directly in methyl-blue, without decolorising with nitric acid, the blue stain seeming to expel the red from everything but the bacilli. Ziehl takes up a position as regards the relation of the bacillus to tubercle midway between the advocates of the specificity of the microbe on the one hand, and absolute doubters on the other. He says, "The demonstration of the tubercle-bacillus *may* be employed in the diagnosis of tuberculous affections; sometimes by this means the differential diagnosis from other affections *may* be made." He is also certain that the absence of the bacillus is not to be absolutely relied on as evidence of absence of tubercle in the lung.

9. Dr. H. Menche (*Vortr. im Niederrhein Ver. f. Nat. u. Heilk*, 22nd Jany., 1883), and Dr. Fr. Crämer (*Erlanger Phys. Med. Sitzgb.*, 11th Dec., 1882) concern themselves in their communication chiefly with the occurrence of a micro-organism in the fæces, which is stained in exactly the same way as the tubercle-bacillus. Dr. Crämer has found it in 20 different stools of *healthy* persons, and regards it as impossible to distinguish it in any way from the genuine tubercle-bacillus. Dr. Menche, on the other hand, considers this observation as founded on error, caused by deficient decolorising of the preparations; he holds that the presence of the true tubercle-bacillus in the intestinal contents, which was noticed by him only in the stools of the really tuberculous, is due to the admixture of the secretions of tuberculous ulcers.

10. Dr. J. Dreschfeld (*Brit. Med. Jour.*, 17th Feby., 1883) employed Rindfleisch's modification of Ehrlich's process (quick staining in a warm solution) in the examination of the sputum of 46 cases of phthisis in which the phthisical signs were clear, and invariably found the bacillus, though in variable quantity. In two out of three doubtful cases, also, it was detected, and these he regarded as tuberculous. In eight non-tuberculous lung affections the bacillus was wanting. He finds, however, that the number of bacilli present in tubercular cases gives no indication as to prognosis, having occasionally found abundance of bacilli with little fever, and few bacilli with high fever.

11. Dettweiler and Meissen (*Berl. Klin. Woch.*, Nos. 7 and 8, 1883) discovered the tubercle-bacillus in 85 out of 87 cases diagnosed clinically as phthisis; in 82 of these elastic fibres were detected. They do not find the number of the bacilli at all proportionate to the gravity or acuteness of the disease. The authors mention several grounds on which phthisis can not be regarded as a purely infectious disease: heredity, the undoubted improvement seen in patients who have for years been phthisical, even in those who have coughed up bacilli, the influence of "stagnation-centres" as soils predisposed to the disease. A local "bacillosis" may continue comparatively harmless so long as the tissues adjoining the colony retain their normal vitality; but with the increase or the addition of enfeebling influences, the danger of the general distribution of the parasite increases. In caseous masses the bacillus seems to increase and develop in special abundance. The communicability of the bacillus, even by the air passages, is not excluded; but there is always presupposed, as an indispensable condition, the existence of a lowered state of vitality of parts of the lungs.

12. Dr. A. Spina gives an account, in the first 76 pages of his pamphlet (*Studien über Tuberculose*, Wien, 1883), of the histology of tuberculosis, and of the various ways in which it may be produced experimentally or conveyed by inoculation, inhalation, or feeding with tuberculous materials. In other 52 pages he endeavours to show that there are bacteria which have no relation whatever to tubercle, and which also show the characteristic staining reaction of Koch; and that he had failed to discover those other external characteristics which are said to distinguish the tubercle bacillus from similar bodies. He failed to find even those first mentioned bacteria in tubercles obtained from parts not in contact with the atmosphere. Finally, basing his attack on a very insufficient investigation, he assails also the inoculation experiments of Koch.

13. Dr. R. Koch (*Deut. Med. Wochensch.*, No. 10, 1883) discusses, amongst other subjects, the assertions made by Spina, who so far seems to be the only investigator who has undertaken to check or verify Koch's recent work in its whole extent. He concludes that everything shows that "Spina does not understand either how to look for, or to cultivate, or to inoculate bacteria. On the literature of the tubercle-bacillus his work has no influence." Koch also opposes Dettweiler and Meissen, and holds firmly to his view of the specificity of the tubercle-bacillus, pointing to the facts that exclusively the same small bacillus has been found in the contents of pulmonary cavities, in miliary tubercle of the spleen, in tuberculous disease of the cerebral membranes, in fungous capsules of joints, and in animals suffering, some from spontaneous and some from experimentally produced tuberculosis.

14. Dr. H. W. Schmidt, of New Orleans, believed he had proved that Koch's bacilli are merely fat crystals, but Dr. Hirschfelder, of San Francisco (*New York Med. Rec.*, 6th Jan., 1883), has found that, after placing the cover-glass in boiling ether, and washing thoroughly with fresh ether, the bacilli could be readily shown by Ehrlich's method.—*Lond. Med. Rec.* 15th April, 1883.

15. Dr. R. Charnley Smith publishes the following letter in the *Brit. Med. Journal*, 20th January, 1883:—"By the following simple method, I have succeeded in demonstrating with facility the presence of the bacilli of tubercle in the breath of patients suffering from true tubercular consumption. For this purpose I allow the patient to breathe at frequent intervals during the day, through two thin sheets of pyroxylin, or gun cotton, one layer in front of the other, and both of which are placed in the outer compartment of an ordinary "pepper-duster" respirator. The layer of cotton, when so arranged, will act as a double filter, the external layer removing from the ingoing air all suspended particles, such as dust, micro-fungi, pollen, starch, &c., which are always more or less present in it, and which it is desirable to exclude, that portion of cotton which has been next to the mouth at the same time retaining those only existing in the outgoing current, and which have been emitted from the lungs —viz., micrococci, bacilli, and some epithelial cells. It is in the latter layer only that I look for the organisms peculiar to this disease. This I do by converting the pyroxyline into collodion by means of a mixture of ether and spirit. Every vestige of cotton fibre is dissolved in the above menstruum, but other organic particles remain suspended in it. To render the bacilli manifest, my plan is to pour the thin

collodion thus formed on a microscope slide, and allow the fluid to run uniformly over the surface of the glass, then immediately placing the latter on one of its edges, that only the merest film of collodion may remain on the glass; the thinner the film produced, the more successful will be the experiment. The film is to be stained. This may be done by one of the methods well known to the profession for staining tuberculous sputum, such as that of Ehrlich or Heneage Gibbes. I have had excellent results from the former, for full details of which I refer to the last edition of Bristowe, in the appendix. The latter method is described in the *British Medical Journal* of 14th October, 1882."

16. The following method of searching for tubercle-bacilli is followed in the pathological laboratory of Prof. Rindfleisch, in Würzburg. A portion of the sputum is pressed between two cover-glasses, which are then separated and dried in the air. A staining fluid is prepared according to Ehrlich's formula; the cover-slip is then passed three times rapidly through the flame of a spirit lamp, the sputum being on the upper side, and then floated in the staining fluid, which is contained in a watch glass, and the latter held over the flame until the fluid commences to vaporise. The cover-slip is then removed, washed in a stream of water, and laid in absolute alcohol which has been slightly acidulated with nitric acid, when the colour will be removed in about ten seconds. The slip is then taken out of the alcohol, again washed in water, dried, and mounted in balsam.—*Wiener Med. Presse.* 24th December, 1882.

17. In No. 5 of the *Centralbl. f. d. Med. Wiss.*, 1883, Professor Rosenstein announces that he has been able to demonstrate the bacilli of tuberculosis in the urine of a patient with urogenital tuberculosis. Lichtheim previously mentioned having found these organisms in the pelvis of the kidney of a patient who had died from the same disease. The case reported by Rosenstein is one of a man aged 37, with a good family history. Up to four years ago he was quite healthy. Then for the first time he complained of pain during, as well as previous to, micturition. Two years ago he noticed, first in the right, later in the left epididymis, a hardness which appeared to be about the size of a small walnut. The testicles were quite free; no swelling of any lymph glands, nor was there present any sign of disease of the lungs. His general condition was good; no fever was present at any time. The patient passed urine in small quantities, about 50 grammes (1½ oz.) at a time. The total quantity in twenty-four hours varied between 800 and 1,660 cctm. (25 to 52 oz.), sp. gr. 1012 to 1018. Urine was of a

pale yellow colour, acid, highly albuminous, cloudy, even when just passed. Whitish flocculi about the size of the head of a pin floated in it. After standing, an abundant sediment of a whitish-grey colour was deposited, composed almost exclusively of pus corpuscles and but very little blood. For examination, the urine was passed into a solution of thymol, and the fluid drawn off after standing twenty-four hours. A drop of the sediment was placed on a cover glass, dried in the gas flame, and treated according to Ehrlich's method. Observations made with Hartnack 3, obj. 9, showed that in the smallest particles described as flocculi, bacilli were present in great numbers. Professor Rosenstein further remarks that the urine preparations require twenty-four hours' immersion in the staining solution, in this respect differing from those of sputa, which can be well seen after only half an hour's immersion. He also considers it important that the urine preparations, after decolorising with nitric acid, should be stained with a watery solution of methylene blue, because, notwithstanding the urine may have been passed into a solution of thymol, the bacteria of decomposition might be present, in which case their blue colour would readily distinguish them from tubercle bacilli.—*Canada Medical Record.* Feb. 1883.

18. The whole of the current (April) issue of the *Practitioner* is devoted to Mr. W. Watson Cheyne's report to the Association for the Advancement of Research, on *The Relation of Micro-organisms to Tuberculosis:* the most recent English monograph on the subject, and the best. It consists of (1) a short account of the literature on tuberculosis so far as it bears on the point at issue; (2) an account of the author's visit to Toulouse and Berlin, and of what he saw there of the methods and work of those chiefly engaged in these investigations; (3) a description of the best methods of staining, with the results of a large experience of various agents used for this purpose; (4) experiments with non-tuberculous materials; (5) experiments with materials supplied by Professor Toussaint, of Toulouse; (6) experiments with Koch's materials; (7) a general review of the whole subject.

The author gives the following formula for Weigert's solution:—

Saturated watery solution of aniline, .	100 ccm.
Sat. alcoh. sol. of fuchsin, methyl violet, &c.,	11 ccm.

In decolorising *sections* he dispenses with the nitric acid; after staining he washes the section in distilled water, immerses it for a moment in alcohol, and then places it in the following solution:—

Distilled water, ...	100 ccm.
Sat. alcoh. sol. of methyl blue, . .	20 ccm.
Formic acid (pure), ...	10 min.

The sections are left in this from one to two hours, and are then treated in the ordinary manner with water, alcohol, and oil of cloves.

The experiments which Cheyne performed to test the asserted induction of tuberculosis by inoculation of non-tuberculous material, and which were done under the best hygienic conditions, with care as to complete isolation of the animals, and with special attention to sources of tuberculous contamination of instruments, &c., gave entirely negative results; not one of the twenty-five animals became tuberculous, though such substances as setons, vaccine lymph from man and the calf, pyæmic pus, cork, hardened tubercle, and worsted thread, were introduced. He has also in several cases excited abscesses by injection of lanthanum, croton oil, and other irritants, these abscesses exhibiting the tendency to caseation so common in abscesses in rabbits, and in no instance has tuberculosis followed. These results are contradictory to those obtained by many former observers; how is this to be explained? The author points out that, when microscopical examination is not made, cheesy masses, not tubercular, might easily be taken for tubercles; even with microscopical examination, the accuracy of the diagnosis would depend greatly on the staining method and the views the observer held as to what constituted a tubercle. He refers to certain structures that are apt to be taken for tubercle: such as cheesy masses in rabbits' livers, due to psorospermiæ; simple inflammatory masses; changes produced in the lungs by the ova of the strongylus vasorum; small masses of lymphatic tissue generally present in the lungs of guinea-pigs, in the immediate vicinity of large vessels or bronchi. It must also be remembered that in the early experiments on this subject the communicability of tubercle by mediate contagion was not recognised, and as the precautions necessary for thorough disinfection of instruments, &c., had not yet been made out, the channels for the possible introduction of specific micro-organisms were left unguarded.

Toussaint cultivates certain organisms (micrococci, not bacilli), from the blood of tuberculous animals, and after repeated cultivations injects these organisms into other animals, producing, as he says, tuberculosis. He regards the micrococcus as the cause of the tuberculosis. Cheyne inoculated seven animals with materials obtained direct from Toussaint, and not one became tuberculous. He also cultivated micrococci obtained from Toussaint, and injected them into twelve animals; of these, seven were under observation for a sufficient length of time without the development of tubercle in any of the animals. Thus, thirteen animals were inoculated with the micrococci with which Toussaint works, obtained from Toussaint himself, and in no case did tuberculosis follow. From these and other facts, Cheyne concludes that the micrococci described by Toussaint do not cause tuberculosis, and therefore his results must be due to some other agency. Toussaint produces true tuberculosis very often, not by means of his micrococci, but from some fault in experimentation, such as contamination of his fluids with Koch's bacilli, or from the presence of these organisms in the air of the room in which the experiments are done. Toussaint also depends largely on carbolic acid as a disinfectant, an agent which is quite ineffectual against the spores of bacilli, unless it acts for a long time.

"In Koch's research, the results are much more definite than any previously obtained. He also cultivates micro-organisms from tubercle, but now it is no longer the fact that he only sometimes succeeds in causing tuberculosis, and that the tuberculosis thus produced occurs as slowly as, or more slowly than, from inoculation of tuberculous material. The result of the inoculation of his cultivations is certain, and the disease is more rapid in its commencement than after inoculation of tuberculous matter. The fact that the inoculation of the cultivated bacilli is so certain and rapid in its effects can only be explained on the supposition that in these cultivations we have the virus of the disease in a more or less pure state, and in large amount. But as the only things which we see multiplying on the serum are these bacilli, and as they are also constantly present in tubercles, it is difficult to see what other conclusion can be come to than that they constitute the virus.

"Professor Klebs makes two objections to Koch's facts, and apparently still upholds his views that a micrococcus and not a bacillus is the real cause of tuberculosis. The first of his objections is that these bacilli may be crystals. This objection is not a formidable one; for any one acquainted with micro-organisms can tell on examination that this is an organism and not a crystal. Its appearance, the apparent presence of spores, its variable lengths, its frequent curved form, its arrangement, its behaviour with staining agents, and above all its growth on serum, demonstrate its living nature. It is difficult to conceive that a few crystals put on serum will grow and extend from one point with the rapidity and in the mode that these bacilli spread, and I know of no better test, in spite of Klebs' assertion to the contrary, for a living organism than its increase and multiplication when placed on a suitable soil. They are not any kind of fat crystals, for they are not found in cheesy matter which is not tubercular; and they vary greatly in number in tubercular material itself, sometimes only one being present in two or three fields of the microscope, sometimes a large number.

"All that has as yet been absolutely *proved* is that a variety of materials in man which we class together as tuberculous, produce, when inoculated into rabbits, guinea-pigs, and other animals, acute tuberculosis, and that this also occurs from the inoculation of bovine tuberculosis. Koch's researches further demonstrate that this result is due only to the tubercle bacilli which were present in the materials inoculated. We may say definitely that the tubercle bacillus is the cause of acute tuberculosis, and that scrofulous glands, degenerated (strumous) synovial membranes of joints, phthisical lungs (in short, all those materials obtained from man which, inoculated into animals produce acute tuberculosis), contain in them bodies (bacilli) which, if they entered the circulation in sufficient numbers, would give rise to acute tuberculosis. It has been demonstrated by several observers that probably in all cases of acute tuberculosis a place can be found where these bacilli get into the circulation.

"Two distinct structures have been described, and may be readily recognised in a tuberculous lung—viz., nodules of lymphatic tissue in close proximity to the vessels and bronchi, and nodules which are largely made up of epithelioid cells. If a case of commencing tuberculosis of the lung be examined, it will be found that bacilli are only present in the latter nodules; and, indeed, it is rare, even in the later stages, to find them in the former, and in that case epithelioid cells will be found as well. The bacillus being the cause of this disease, only the nodules containing epithelioid cells are tubercles. Surrounding the epithelioid cells, which are always in the central portion of the tubercle, and make up the greater part of it (though after a time leucocytes penetrate among them), we have what I consider to be simply inflammatory tissue, but what is sometimes spoken of as lymphatic tissue. As the tubercle gets older, it is found that the epithelioid cells at the centre undergo cheesy degeneration, and they can only be seen, if present at all, at the margin. In tubercle we also find giant cells in which bacilli are generally present, sometimes in considerable numbers. These giant cells I have distinctly traced to epithelioid cells, especially to epithelioid cells containing bacilli; for where several bacilli are present in cells, all gradations may be found between the single nucleated cell and the multinucleated giant cell.

"As to the origin of these epithelioid cells, I can only speak from a study of the process in the lungs and the liver. In the lungs, I am satisfied that the great majority are derived from the alveolar epithelium. The bacilli escape from the blood-vessels or lymphatics, and get into the alveolar epithelium, where they grow and cause multiplication of the epithelial cells, till the alveolus becomes completely filled with these cells and infiltrated leucocytes. Around this mass the walls of the alveolus become inflamed and thickened, and form the granulation tissue surrounding the epithelioid mass.

"The structural definition of a tubercle must run somewhat as follows:—A nodule, composed of a central mass consisting in the main of epithelioid cells, or in its place a cheesy mass, surrounded by more or less inflammatory tissue, with or without the presence of giant cells. The absolute diagnostic mark is, however, the presence of the tubercle bacillus. A group of granulation cells without epithelioid cells, or without cheesy matter or giant cells to indicate the previous presence of epithelioid elements, is not a tubercle. On the other hand, I know of no morbid structure except tubercle which contains the same histological elements, arranged in the same way, and possessing the same tendencies.

"The following is what the foregoing facts lead me to suppose to be the sequence of events in phthisis. The tubercle bacilli which reach the lung by inhalation develop in the epithelial cells lining an alveolus, this alveolus becomes filled with cells, neighbouring alveoli become infected, and the same process goes on in them. The further result will depend on the number and rapidity of growth of the bacilli, and on whether the patient is a good soil for their development. If they develop well, we have caseous pneumonia, if they grow slowly and with difficulty, we have fibroid phthisis. In the former case the alveoli become distended early with epithelioid cells, this leads to inflammation of the walls of the alveoli, the cells soon undergo cheesy degeneration, and the pressure of the masses leads to atrophy or sloughing of the walls of the alveoli. (In the latter case elastic tissue will be found in the sputum.) Infection of neighbouring parts of the lung occurs both by continuity, and also by partial coughing up and re-inhalation of the bacilli into other parts of the lung. In this rapid phthisis, fibrous formation around the alveoli only takes place imperfectly, and the lung rapidly breaks down. In the case of fibroid phthisis the bacilli are few, and grow only with difficulty. Thus fibrous formation occurs extensively, and giant-cells are caught in this fibrous tissue in the manner formerly described. Nevertheless, in parts the process may be more rapid, and there cheesy masses are formed which may lead to breaking down of the lung and the formation of cavities. On this view we have an explanation of several facts. First, we have the rarity of acute general tuberculosis in connection with phthisis, even though bacilli are present in the lungs. One reason of this is probably that the bacilli can hardly be said to enter the body: they are separated from the circulation by the layer of granulation and fibrous tissue. This is a fact which can be readily observed. Secondly, we have the explanation of their presence in the sputum even before physical signs are marked, or, indeed, have become evident at all. It is now stated that the number of bacilli in the sputum is a means of forming a prognosis as to the rapidity of the disease. According to the views I have just stated, this would be a very likely thing, for the presence of large numbers in the sputum would indicate either an affection of numerous alveoli, or a large amount of caseous material—*i. e.*, extensive affection of one part of the lung, and hence rapid death.

"A consideration of all the facts has led me to the conclusion that tuberculous processes in the lungs are due to the tubercle bacilli, and, so far as I know, to them only. By a tuberculous process I mean one where there is proliferation of epithelium, caseous degeneration of this proliferated epithelium, and inflammation round about, these changes being progressive. It has been supposed that inhalation of dust of various kinds may give rise to phthisis. That the inhalation of dust will lead to inflammatory changes is very likely, that it may lead to proliferation of epithelium which may subsequently degenerate is possible, but that the process will be progressive and extend beyond the seat of irritation is not probable. That the changes set up by the presence of gritty particles may, however, prepare the lung and render it a fit soil for the implantation of bacilli is very probable, and in this way a true tuberculous process may supervene, not due to the original gritty substances, but to the bacilli which came afterwards. It has often been urged that the milk of tuberculous cows is infective. This may be the case when the mammary glands become tuberculous."

Mr. Cheyne is to be congratulated on the production of this very able monograph; it is well worked out, and we commend it to the attention of our readers.

THE

GLASGOW MEDICAL JOURNAL.

No. VI. June, 1883.

ORIGINAL ARTICLES.

MINUTE ORGANISMS AND THEIR RELATION TO DISEASE.

By ROBERT MUNRO, M.A., M.D., Kilmarnock.

Part II.

The splendid success of these researches of Pasteur could not fail to throw much light on the general pathology of epidemic diseases among the higher animals, and the immediate result was that the germ theory, formerly only occasionally mooted as a mere speculation, now again came to the front. A disease known under the various names of splenic fever, charbon, anthrax, &c., was then causing much animal loss in several countries in Europe. In its most malignant form horses, cattle, and sheep, were killed in 24 hours. Even man himself was not safe from its attacks, as we find from the statistics of the district of Novgorod in Russia, where no less than 56,000 animals and 528 human beings are said to have died of its effects during the three years preceding 1870.

As early as 1850 two French pathologists, MM. Davaine and Rayer, noticed, in the blood of animals which had died of splenic fever, certain little rod-like bodies, but no causal connection between these microscopic organisms and the disease was thought of till the publication of Pasteur's memoir on the fermentation of butyric acid in 1861, when it occurred to Davaine that splenic fever might also be a kind of fermentation set up in the living body. It was then that the life history of these rod-like bodies, or as they are now called *bacilli anthracis*, became an important question for investigation. The problem was subsequently worked out by Dr. Robert Koch, then a country practitioner of Wollstein, in Germany, whose careful researches are thus described by Professor Tyndall:—

"He studied the habits of the rod-like organisms, and found the aqueous humour of an ox's eye to be particularly suitable for their nutrition. With a drop of the aqueous humour he mixed the tiniest speck of a liquid containing the rods, placed the drop under his microscope, warmed it suitably, and observed the subsequent action. During the first two hours hardly any change was noticeable; but at the end of this time the rods began to lengthen, and the action was so rapid that at the end of three or four hours they attained from ten to twenty times their original length. At the end of a few additional hours they had formed filaments, in many cases a hundred times the length of the original rods. The same filament, in fact, was frequently observed to stretch through several fields of the microscope. Sometimes they lay in straight lines parallel to each other; in other cases they were bent, twisted, and coiled into the most graceful figures; while sometimes they formed knots of such bewildering complexity that it was impossible for the eye to trace the individual filaments through the confusion.

"Had the observation ended here, an interesting scientific fact would have been added to our previous store; but the addition would have been of little practical value. Koch, however, continued to watch the filaments, and after a time noticed little dots appearing within them. These dots became more and more distinct, until finally the whole length of the organism was studded with minute ovoid bodies, which lay within the outer integument like peas within their shell. By and bye the integument fell to pieces, the place of the organism being taken by a long row of seeds or spores. These observations, which were confirmed in all respects by the celebrated naturalist, Cohn of Breslau, are of the highest importance. They clear up the existing perplexity regarding the latent and visible contagia of splenic fever, for in the most conclusive manner Koch proved the spores, as distinguished from the rods, to constitute the contagium of the fever in its most deadly and persistent form.

"How did he reach this important result? Mark the answer. There was but one way open to him to test the activity of the contagium, and that was the inoculation with it of living animals. He operated upon guinea pigs and rabbits, but the vast majority of his experiments were made upon mice. Inoculating them with the fresh blood of an animal suffering from splenic fever, they invariably died of the same disease within twenty or thirty hours after inoculation. He then sought to determine how the contagium maintained its vitality. Drying the infectious blood containing the rod-like organisms, in which, however, the spores were not developed, he found the contagium to be that which Dr. Sanderson calls "fugitive." It maintained its power of infection for five weeks at the farthest. He then dried blood containing the fully developed spores, and exposed the substance to a variety of conditions. He permitted the dried blood to assume the form of dust; wetted this dust, allowed it to dry again, permitted it to remain for an indefinite time in the midst of putrefying matter, and subjected it to various other tests. After keeping the spore-charged blood which had been treated in this fashion for four years, he inoculated a number of mice with it, and found its action as fatal as that of blood fresh from the veins of an animal suffering from splenic fever. There was no single escape from death after inoculation by this deadly contagium. Uncounted millions of these spores are developed in the body of every animal which has died of splenic fever, and every spore of these millions is competent to produce the disease." (*Lecture on Fermentation*, 19th Oct., 1876. Pp. 30-32).

But Koch was not the only observer who reaped laurels in this interesting line of research. Already eminent professors and pathologists in various parts of Europe were eagerly watching not only splenic fever, but the phenomena of all other epidemic diseases, in light of Pasteur's marvellous discoveries. Amongst them, however, Pasteur himself still kept the foremost place. Hearing that certain localities were liable to sudden, frequent, and unaccountable outbreaks of splenic fever in its most deadly form, he instituted inquiries as to the disposal of the dead bodies of its victims, and found that they had been deeply buried in the fields where the animals died. Notwithstanding that the carcases had lain there for several years, and at a depth of from ten to twelve feet, Pasteur ascertained that the grass over their graves contained the germs of this disease, and that these germs were actually carried upwards through the soil by the agency of earthworms. Of this he gave experimental proof. Collecting some earthworms from the infected spot he made an extract of the contents of their alimentary canals, and found that by inoculating rabbits and guinea pigs with this extract, they became affected with the severest form of the disease.

Professor Burdon Sanderson supplies another most interesting fact regarding the mode in which this disease may be propagated. While investigating into the cause of a simultaneous outbreak among a number of herds widely separated from each other, he ascertained that the affected cattle had all been supplied with *brewer's grains* from a common source, which, on being examined with the microscope, were found to be swarming with the bacillus anthracis.

A peculiar and sometimes rapidly fatal disease, known among sorters of alpaca wool and mohair in the Bradford district, and to which they have been liable for many years, is now known to be nothing else than a modification of splenic fever or anthrax, regarding which Dr. Bell of that town writes as follows:—"During the last few months, I have seen several cases of external anthrax in persons who have come into contact with mohair or dry wools. This has not previously been associated with 'wool sorters' disease;' it is, however, the local or external form of this disease, and is caused by the introduction of the poison at the part affected. The constitutional form of anthrax, as it affects wool sorters, is not generally attended by external or internal pustule. It is a general blood disease caused by the introduction into the circulation of the spores of the *bacillus anthracis* derived from the fleeces of animals which have died from this disease. As there is no anthrax, I have called the disease anthracæmia. It is sometimes epizootic in the country where these hairs and wools are grown. It affects persons who come into contact with the animals; but their illness and rapid death are not attributed to this cause. The blood of a person suffering from wool sorters' disease or anthracæmia, when injected under the skin of a rabbit or other animal, produces death in two or three days; and the blood of these, when examined shortly after death, sometimes swarms with the bacillus anthracis." (*Brit. Med. Journal,* 23rd October, 1880.)

In the course of his exhaustive researches on alcoholic fermentation, Pasteur discovered that the potency or energy of the ferments became gradually diminished when allowed to germinate in wort having free access to the air. This curious phenomenon in the life history of the torula probably suggested to Pasteur's fertile imagination to inquire if the vitality of these microbes of disease was also liable to a similar modification. This he has accomplished by cultivating them in certain liquids—such as blood serum or meat juice, which were found to be favourable to their growth—having free access to purified air, and then testing the effects of this cultivated virus on animals by inoculating them with it at regularly increasing intervals of time. Before, however, following Pasteur's researches on this point, let us glance at the work done by some of the other distinguished pathologists who were attracted to the study of this most interesting field of inquiry.

In March 1878, at the suggestion of Dr. Burdon Sanderson, a calf was inoculated with the blood of a guinea pig which had died of splenic fever, with the view of ascertaining whether the potency of the virus became diminished by its passage through the blood of the latter animal. The result was that the calf took the disease in a mild form and recovered. Following this clue, Dr. Greenfield repeated the experiment, and confirmed the accuracy of this inference and, moreover, came to the conclusion, that the inoculation of bovine animals with the blood of rodentia, affected with splenic fever, not only ensured a mild form of the disease, but also conferred immunity upon the cattle so treated from natural infection. In proof of this, three animals protected by primary inoculation were sent to Bradford, "where they were placed on sewage meadow, on which splenic fever occurred just before and during the time the animals were there, and which was irrigated with water in which wool infected with anthrax had been washed; but they remained there perfectly healthy for four or five months, and were removed in good condition. When the owner of the meadow noticed that these animals remained healthy, he ventured to put some of his own stock on again; but, no sooner had he done so, than one of them died of splenic fever."

Professor Toussaint, of Toulouse, has investigated this question from a somewhat different standpoint. Dubious as to whether it was the presence of the micro-organisms, or some chemical product of their vital action in the blood, that conferred immunity upon animals from further attacks, he performed the experiment of inoculating healthy sheep with blood from one just dying of splenic fever, after previously depriving the blood of the bacillus anthracis. Its removal from the infected blood was effected in two ways, either by filtration, or by subjecting the blood to a temperature of 131° F., an amount of heat which was known to destroy the life of this microbe. The inference drawn from his experiments by Professor Toussaint was that a free injection of the diseased blood, treated in either of the above ways, carried with it a true "vaccine" influence which protected the animals from the natural disease. He notes, however, as a fact worthy of consideration, that this protective influence was not produced all at once, but that it took some weeks ere the animals were safe from a re-inoculation with the active virus.

Dr. Hans Buchner, of Munich, working under the directions of Professor Nägeli, maintains that, by a series of ingenious methods of cultivation in albuminous fluids, he has succeeded in converting a very innocuous organism called hay bacillus, *B. subtilis* (because it is frequently met with in hay infusions), into the bacillus anthracis and *vice versa*. This is a very curious statement, because, though morphologically these two micro-organisms are similar, physiologically they are very different and require totally different conditions of life. Hence, Dr. Buchner holds that the power of the *B. anthracis* in producing disease, though greatly enfeebled by successive cultivations, is never absolutely lost, but only held in abeyance, and when cultivated in another medium its virulent properties may be restored. Another observer, Dr. Grawitz, asserts that some of the most innocent microphytes may be changed by artificial culture into the most deadly disease germs. Pasteur has also found that, by inoculating a young guinea pig with a virus mitigated by cultivation, and then an older one from it, and repeating the process several times, the original potency of the virus can be restored and even intensified in some instances. The significance of these observations, as throwing light on the degree of severity by which various epidemic diseases are characterised, and the mode by which a mild outbreak may be converted into one of a malignant type, or *vice versa*, must be apparent to all.

Another disease known on the Continent under the name of *cholera des poules*, has been specially investigated by Pasteur, Toussaint, and others, with results still more definite than those deduced from the phenomena of splenic fever. Fowl cholera, though a highly infectious blood disease, has no resemblance to true cholera, having got the name merely because it raged in the poultry yards of Paris, when the latter was prevalent in the city. Its chief symptoms are great swelling of the glands of the neck, pericarditis with effusion, and sometimes ulceration of the duodenum. Pasteur found that the microbe associated with this disease could be readily cultivated in meat juice, but most advantageously in chicken tea, and that, when successively cultivated at short intervals, it retained its virulence for any length of time. When, however, the organism was allowed to remain for a considerable time in a given quantity of tea, it ceased to develop further; moreover, fresh virus introduced into this same tea would not grow, though it still continued to afford an excellent pabulum for the development of other forms of bacteria. He also ascertained, as in the case of the ferments, that its potency could be modified to such a degree that healthy fowls, when inoculated with the cultivated virus, suffered only from a mitigated form of the complaint which, however, rendered them secure against the fatal form of the disease.

At the International Medical Congress, held in London, in August, 1881, Pasteur delivered an address on "Vaccination in relation to Chicken Cholera and Splenic Fever," in which he describes the method of preparing "vaccine" used in preventing these diseases. His investigations on chicken cholera have been attended with more definite results than those on splenic fever, owing to greater difficulty in modifying the virus of the latter disease, in consequence of certain peculiarities in the physiological development of its microbe—preferring to multiply by spores rather than by fission. The principle, however, on which the mitigation of the virus in both cases depends is virtually the same. On the culture of the virus of fowl cholera, the following are Pasteur's words:—

"Let us take, then, a fowl, which is about to die of chicken cholera, and let us dip the end of a delicate glass rod in the blood of the fowl with the usual precautions, upon which I need not here dwell. Let us then touch with this charged point some *bouillon de poule*, very clear, but first of all rendered sterile under a temperature of about 239° Fah., and under conditions in which neither the outer air nor the vases employed can introduce exterior germs, those germs which are in the air or on the surface of all objects. In a short time, if the little culture vase be placed in a temperature of 77° to 95° Fah., you will see the liquid become turbid and full of tiny micro-organisms, shaped like the figure 8, but often so small that, under a high magnifying power they appear like points. Take from this vase a drop as small as you please, no more than can be carried on the point of a glass rod as sharp as a needle, and touch with this point a fresh quantity of sterilised *bouillon de poule* placed in a second vase, and the same phenomenon is produced. You deal in the same way with a third culture vase, with a fourth, and so on to 100, or even 1,000, and invariably within a few hours the culture liquid becomes turbid and filled with the same minute organisms. At the end of two or three days' exposure to a temperature of about 86° Fah., the thickness of the liquid disappears, and a sediment is found at the bottom of the vase. This signifies that the development of the minute organism has ceased—in other words, all the little points which caused the turbid appearance of the liquid have fallen to the bottom of the vase; and things will remain in this condition for a longer or shorter time, for months even, without either the liquid or the deposit undergoing any visible modification, inasmuch as we have taken care to exclude the germs of the atmosphere. A little stopper of cotton sifts the air which enters or issues from the vase through changes of temperature."

We might here expect that the strength of this plague-poison would be gradually weakened as we increased the number of cultures; but, strange to say, the virulence of each successive culture remains exactly the same, and as potent as the original virus from the blood of the fowl. This equality in the virulence of both the culture preparation and of the blood is due, according to Pasteur, to the trivial circumstance of not "leaving any considerable interval between the impregnations." "Let us now repeat," he goes on to say, "exactly our successive cultures,' with this single difference, that we pass from one culture to that which follows it, from the 100th to, say the 101st, at intervals of a fortnight, a month, two months, three months, or ten months. If, now, we compare the virulence of the successive cultures, a great change will be observed. It will be readily seen from an inoculation of a series of ten fowls, that the virulence of one culture differs from that of the blood and from that of a preceding culture when a sufficiently long interval elapses between the impregnation of one culture with the micro-organism of the preceding. More than that, we may recognise by this mode of observation that it is possible to prepare cultures of varying degrees of virulence. One preparation will kill eight fowls out of ten. another five out of ten, another one out of ten, another none at all, although the micro-organism may still be cultivated. . . . .

"A little further, and we touch the principle of vaccination. When the fowls have been rendered sufficiently ill by the attenuated virus, which the vital resistance has arrested in its development, they will, when inoculated with virulent virus, suffer no evil effects, or only effects of a passing character. In fact, they no longer die from the mortal virus, and for a time sufficiently long, which in some cases may exceed a year, chicken cholera cannot touch them, especially under the ordinary conditions of contagion which exist in fowl houses. At this critical point of our manipulation—that is to say, in this interval of time which we have placed between two cultures, and which causes the attenuation, what occurs? I shall show you that in this interval the agent which intervenes is the oxygen of the air. Nothing more easily admits of proof. Let us produce a culture in a tube containing very little air, and close this tube with an enameller's lamp. The micro-organism, in developing itself, will speedily take all the oxygen of the tube and of the liquid, after which it will be perfectly free from contact with oxygen. In this case it does not appear that the micro-organism becomes appreciably attenuated, even after a great lapse of time. The oxygen of the air, then, would seem to be a possible modifying agent of the virulence of the micro-organism of chicken cholera; that is to say, it may modify more or less the faculty of its development in the body of animals. May we not be here in presence of a general law applicable to all kinds of virus? What benefits may not be the result? We may hope to discover in this way the vaccine of all virulent diseases."

Bearing in mind Koch's observations on the contagium of splenic fever, how the micro-organisms developed themselves by spores or germs in their interior, how these spores only retained the power of propagating the disease after a certain time, and how the corpuscles, or rod-shaped organisms, exactly corresponded with what Dr. Sanderson calls the "fugitive" form of the contagium, we can readily form some idea of the greater difficulty encountered in the process of mitigating the virulence of the contagium of splenic fever, so as to make it a safe prophylactic vaccine. According to Pasteur's theory, it is necessary that the disease corpuscles should be in contact with oxygen for a considerable time before this degree of safety is attained; hence the primary difficulty with B. anthracis is the rapidity with which it develops its spores, thereby not giving, as it were, sufficient time to the oxygen to produce its benign influence. This is how Pasteur has got over this difficulty.

"A great difficulty presents itself when we attempt to apply our method of attenuation by the oxygen of the air to the anthracoid micro-organisms. The virulence establishing itself very quickly, often after four and twenty hours in an anthracoid germ, which escapes the action of the air, it was impossible to think of discovering the vaccine of splenic fever in the conditions which had yielded that of chicken cholera; but was there, after all, reason to be discouraged? Certainly not. In fact, if you observe closely, you will find that there is no real difference between the mode of the generation of the anthracoid germ by fission and that of chicken cholera. We had, therefore, reason to hope that we might overcome the difficulty which stopped us by endeavouring to prevent the anthracoid micro-organism from producing corpuscle germs, and to keep it in this condition in contact with oxygen for days, and weeks, and months. The experiment fortunately succeeded. In the ineffective *bouillon de poule* the anthracoid micro-organism is no longer cultivable at 45° Cent. Its culture, however, is easy at 42° or 43° Cent., but in these conditions the micro-organism yields no spores. Consequently, it is possible to maintain in contact with the pure air, at 42° or 43° Cent., a mycelian culture of bacteridia entirely free of germs. Then appear the very remarkable results which follow. In a month or six weeks the culture dies. That is to say, if one impregnates with it fresh *bouillon*, the latter is completely sterile. Up till that time life exists in the vase exposed to air and heat. If we examine the virulence of the culture at the end of two days, four days, six days, eight days, &c., it will be found that long before the death of the culture the micro-organism has lost all virulence, although still cultivable. Before this period it is found that the culture presents a series of attenuated virulences. Everything is similar to what happens in respect to the micro-organism in chicken cholera."

To prove that by this method of artificially cultivating the *bacillus anthracis* a safe vaccine could be produced, capable of protecting cattle from the ravages of splenic fever, Pasteur had again resorted to the expedient of demonstrating its efficacy by a public experiment, having previously predicted the result. This demonstration took place only a few weeks before he delivered the address from which I have quoted the above passages, on the farm of a veterinary surgeon at Pouilly-le-Fort. Sixty sheep were placed at his disposal, ten of which were untouched, in order that they might later on serve for comparison:—

"Of the remaining fifty twenty-five were marked with a hole in their ears, and were inoculated, the first time on the 5th of May, and the second on the 17th. On 31st May, none of the inoculated sheep had lost fat, or spirits, or appetite. On 31st May, the fifty sheep were taken, without distinction, and inoculated with the strongest virus. M. Pasteur predicted that, on the 2nd inst., the twenty-five sheep not inoculated would be dead, and that the inoculated animals would show no symptoms of sickness; and, accordingly, on that day, a number of spectators, including the President of the Agricultural Society of Melun, the Prefect of the Department, and the Director of Agricultural matters at the Ministry of Agriculture and Commerce, assembled to witness the result. The prophecy of M. Pasteur was exactly fulfilled. At two o'clock, twenty-three of the sheep which had not been inoculated were dead. At three o'clock died the twenty-fourth, and the twenty-fifth an hour later. The twenty-five inoculated animals were quite sound, and in perfect health. Only one of them was feverish; and the fever, which was caused by the animal having designedly been inoculated with too strong a dose of the virus, speedily disappeared."—*Brit. Med. Journal.* 11th June, 1881.

These brilliant results, which inspire Pasteur with boundless anticipations for the future, prove, according to him, "that we are in possession of a general method of preparing virus-vaccine based upon the action of oxygen and the air—that is to say, of a cosmic force existing everywhere on the surface of the globe."

Considering the stringent atmospheric conditions, and the great delicacy of manipulation requisite in carrying out the method recommended by Pasteur, together with the indefiniteness of our knowledge as to how much of the remarkable changes observed in the developmental stages of *B. anthracis* is due to its own proper life, and how much to other causes, such as the accidental presence of other microbes, it could hardly be expected that other physiologists, working in the same difficult field of labour, should arrive at precisely similar results, and accept his theory without question. Notably amongst those, in this country, who have failed, either to procure an attenuated virus after Pasteur's recognised method of successive cultivation in meat juice, or to verify the efficacy of his specially prepared "vaccin charbonneux," is Dr. Klein. This observer agrees with Koch and Pasteur as to the extreme virulence of the spores of the *B. anthracis*, but thinks that their formation or non-formation is largely a matter of definable circumstances and conditions. He also thinks that, after several weeks' growth, this bacillus undergoes degeneration from exhaustion "of the pabulum on which it has lived," which ultimately destroys its morbific qualities; and if any effect at all be produced by inoculation of this degenerate material, it is just the same result as would have been produced on the animal by inoculation of the material before the degenerative process had begun in it. He admits therefore of no attenuation or variation in the degree of the potency of the virus.

In a communication to the Veterinary Department of the Privy Council, at the beginning of October of last year, regarding a series of experiments he performed with "*vaccin charbonneux*," obtained from M. Bontroux, the agent of M.

Pasteur in Paris, Dr. Klein says—"The results of these experiments enable me to say: (*a*) Animals inoculated with this 'vaccin' (premier and deuxième) are not made immune against fatal anthrax; and (*b*) both the first and second vaccin may produce fatal anthrax."

Upon this communication being brought under the notice of M. Pasteur, that eminent savant wrote as follows:—

"I am much obliged to you for having informed me of the result of Dr. Klein's experiments on *vaccin charbonneux*. These experiments suggest to me the following reflections:—The discovery of a means of producing modified virus, and the efficiency of the practical use of such virus in all matters relating to *vaccin charbonneux* have been so thoroughly proved, in various countries, for at least a year, that there is nothing more to be done, in the way of demonstration, to settle the question. It is an accomplished fact that now belongs to science.

"If any experimenter, however competent he may be, and I know that Dr. Klein is very able, fail when he attempts a fresh verification of the question, his duty, in the present condition of science, should be the investigation of the motives by which he is opposed. I have already pointed out that the question of species and race should be taken into consideration. A vaccine which vaccinates rabbits vaccinates sheep very badly, or to a very small extent. Some breeds of sheep do not tolerate at all, or tolerate very badly, the vaccine which is efficient for another breed. It is advisable that preliminary trials should be made on a small number of individuals, especially if we pass from one country to another, so that we may be assured of the degree of strength of the vaccine, and if it be desired, also to find out what is suitable to new species or varieties; mice and guinea pigs cannot be used for the trial of sheep vaccine. There are also, in the kind of experiment under consideration, other sources of possible failure. Thus, I have already pointed out, that in one of the experiments undertaken in Italy, the virulent virus employed was septicæmic as well as anthracic. The vaccinated animals died as well as the non-vaccinated subjects.

"Dr Klein has, as it appears to me, equally failed in proving that *vaccin charbonneux* loses its virulence, when cultivated at 42° to 43° Cent., in contact with air; this is because he has not followed the method pointed out by me, and here also he can only blame himself for his failure. The conclusion of the communication to which I am now replying is prudent to the last degree, for it is certainly most inadvisable to introduce *vaccin charbonneux* into a country where anthrax does not exist."—*Brit. Med. Journal.* 14th Oct., 1882.

The position which Dr. Klein has thus assumed in regard to Pasteur's theories and the consequent system of vaccination of animals now extensively carried on in Europe is, that, without doubting his discovery of a material prophylactic against fatal anthrax or splenic fever, he believes there is some additional factor involved in the production of the *vaccin charbonneux*, besides the recognised method of successive cultivations in organic fluids at the temperature of 42° Cent., the conditions of which, however, have not yet transpired from M. Pasteur's laboratory.

Dr. Koch also makes a strong indictment against Pasteur's method of experimentation and "hasty generalisation" regarding the attenuation of virus. While admitting that to him belongs the great merit of having proved that, in certain conditions, the pathogenic microbes of anthrax can be so cultivated as to lose their pathogenic qualities without undergoing any morphological change, he denies the practical utility of vaccination by the attenuated virus thus produced as a preventive of the natural disease; neither does he agree with Pasteur's theory as to the cause of the attenuation of the virus, when cultivated at a temperature of 42·5° Cent., attributing it rather to high temperature than to the oxygen of the air.

The paramount question therefore in medical science is to determine whether the so-called infectious diseases are due to micro-organisms in the living body. Already numerous replies in the affirmative have been given to the question by eminent observers both in Europe and America; so much so, that a sanguine hope is expressed on all hands that the *bacilli* peculiar to all diseases may soon be recognised, and that some means of preventing their ravages in the human constitution will speedily follow—such as an appropriate germicide, or some material which will either minimise their morbid action, or counteract it altogether. That the germs of disease are of a specific nature we have a presumptive evidence in the uniformity and preciseness of their general symptoms when congregated together in the animal system. However much one attack of disease may differ from another, there is no instance of the virus of one giving rise to that of another: the virus of small-pox or scarlet fever produces small-pox or scarlet fever, and never anything else.

Among the more recent researches into the etiology of disease, there are none more interesting than those of Professor Koch regarding the nature of consumption. On the

24th March, 1882, Koch announced the discovery of a bacillus which, he maintains, is not only always associated with tubercle, but the cause of all tubercle, whether in man or the lower animals. By cultivating these bacilli (taken from the most diverse tubercular substances) in blood serum kept free from all other micro-organisms, he showed, by a series of 13 experiments, that they were capable of developing tubercle in all the animals inoculated with them.

By pursuing the method of investigation here adopted by Koch—viz., of first discovering the micro-organism in the morbid tissues and secretions of the diseased animal, and then testing the value of his discovery by inoculations of the virus after cultivation, Fehleisen, assistant to Professor Bergmann, has been enabled to demonstrate that erysipelas is due to the vital action of micrococci in the diseased skin.

From the results of similar researches in the hands of many able investigators, there can be no doubt that many other diseases such as septicæmia, leprosy, &c., as well as our common epidemic diseases are entirely dependent on the presence and functional activity of specific micro-organisms. Considering the opposition that has sprung up in this country within recent years, against the practice of vaccination as a preventive of small-pox, Pasteur's discoveries have been made at a very opportune time. When Jenner ascertained that the degree of protection afforded by vaccination against small-pox was about equal to that of "inoculation," or its equivalent, a first attack of the disease itself, against its recurrence, he suspected that cow-pox might be really small-pox modified by its passage through the tissues and blood of the cow. Indeed, upwards of forty years ago, a Dr. Thiele asserted that he produced vaccinia by inoculating bovine animals with the virus of true small-pox, and not only so, but that the same result could be obtained by diluting the small-pox virus with warm milk. Again, it has been surmised that vaccination does not now confer the same amount of immunity against small-pox that it did when first introduced, and that its protective influence is gradually weakened by human contamination, in proportion to its remoteness from the original bovine stock. Hence, many able practitioners, who have devoted attention to the subject, assert that, in order to preserve the efficacy of vaccination, it is necessary, occasionally at least, to substitute calf lymph for humanized lymph. The effects of inoculating cattle with anthrax-blood of rodents, as indicated by the experiments of Sanderson and Duguid, Greenfield, Klein, Pasteur, Toussaint, and others, as well as the influence of

Pasteur's "vaccin" on sheep have been so suggestive, that the question of the relation between cow-pox and small-pox has again been revived; and I am bound to say that, so far as I can judge, the tendency of the whole evidence is in favour of Jenner's opinion.

Before quitting the new departure thus given to the etiology of disease, we have yet a word to say on the marvellous fact that the most virulent microbes of disease may, by a mere process of horticulture, like plants in a hot-house, be so cultivated and tamed as to lose their malignant qualities without losing their identity. And yet, after all, there is nothing new in the principle. The domestication of animals and the culture of our most prized esculent plants and fruits, from their wild types, though dating back to the dim vista of prehistoric ages, are precisely parallel phenomena. If the botanist can manufacture varieties of flowers at will, and the naturalist multiply his breeds by the score, why should the scientist not succeed in acquiring the same power over these infinitely minute organisms which are well characterised as belonging to *a third kingdom*, and "which, with all the prerogatives of animal life, do not need air to live, and which find the heat they require in the chemical decompositions they create around themselves?" If the vital influence of the cow is such as to alter the virus of small-pox that its introduction into the human subject is no longer attended by deadly consequences—a phenomenon which cannot be denied—may it not have a similar effect on the virus of other equally dreaded diseases? If not, perhaps some other animal may possess the desired influence. Why is it that one attack of disease generally confers immunity from further attacks during one's life time? Is it due to a chemical change in the blood, caused by the vital action of the disease germs? Or have these germs, after an attack (say of scarlet fever), ceased to live because they have exhausted the pabulum necessary to their existence, and so removed some element from the constituents of the blood, which for a long time afterwards renders it an unsuitable *habitat* for the development of similar organisms? But, whatever this change may be, it is not beyond the bounds of hopeful anticipation that it may be produced by artificial means, without any risk to human life. That successive vaccinations of the cultured and attenuated virus of a disease, made on the plan of gradually increasing its potency, may ultimately confer complete immunity from the most virulent type of the natural disease, seems to us not less philosophically probable, than that one can be trained, by gradual doses, to quaff off a few ounces of laudanum with perfect impunity.

Another line of inquiry which promises unlimited scope for practical research, has hardly yet reached the stage of scientific investigation, *i. e.*, to determine what drugs are destructive to the microbes of disease, and to what extent, with safety, such drugs can be introduced into the blood of the affected patient. Thus, Fehleisen found that a 3 per cent solution of carbolic acid stopped the growth of the micrococci of erysipelas after a contact of 45 seconds, whilst the same effect was produced in 15 seconds by a solution of 1 per cent of corrosive sublimate.

What a splendid field for research is thus opening up to the medical profession! Let us hope that empiricism, after all, may be entitled to some niche in the Temple of Æsculapius, by giving birth to a truly scientific system of medical therapeutics. It is therefore no idle dream, in these days of scientific progress, to indulge the hope that the day is not far distant when epidemic diseases will be rooted out of the country, or, at least, become so handicapped that their presence will no longer be a scourge to the community. Man, however, is no exception to the life struggle which everywhere characterises organic nature. Though, by dint of reason, he has no longer to dread the ferocious wild beasts against which his prehistoric forefathers contended in mortal combat, his strategic powers are at the present time taxed to the very utmost in devising the means of successfully opposing those still more deadly foes which, in the garb of insignificant and almost invisible germs, so persistently dog his footsteps in the onward march of civilization.

VACCINAL ERUPTIONS.

By ALEX. NAPIER, M.D.,

Assistant to the Professor of Materia Medica, Glasgow University;
Physician to the Skin Department, Anderson's College Dispensary.

(*Read before the Southern Medical Society, 22nd February, 1883.*)

SKIN affections, as associated with vaccination, have only comparatively recently been very fully discussed by dermatologists. The standard textbooks make very little reference to eruptions having such a special association. The occasional occurrence of generalised vaccinia, named by Neumann "vaccinola," and by Fox "bastard vaccine"—that is, the appearance of a greater or less number of characteristic vaccine vesicles scattered over the body, contemporaneously with those at the points at which the lymph was inserted; the supervention of erysipelas, roseola, eczema, and rarely syphilis; are the chief points hinted at by the principal authorities. But as a rule no attempt is made to classify and arrange them, or to show in what special relation they stand to vaccination.

In France this matter has received the attention of M. Hervieux, who, in presenting his report on vaccination for 1878-79, to the French Academy of Medicine, expressed these views regarding multiple vaccinal eruptions:—

1. Vaccinal eruptions are of two kinds: those caused by accidental inoculation, as by the nails, accidental pricks with the vaccination instrument, &c.; and those resulting spontaneously, as in the exanthemata, from general infection of the system.

2. Of the spontaneous eruptions, some are primary, appearing simultaneously with the vaccine vesicle proper; others are secondary, developing at a later period, usually the ninth or eleventh day. Such a primary eruption presents most commonly the same characters as the ordinary vaccine vesicle; while that developed later assumes very various forms—erythematous, miliary, vesicular, papular, &c.

A very full and interesting discussion of the subject of vaccinal eruptions was presented at the International Medical Congress in London, in 1881, by Dr. Gustav Behrend,* of Berlin, a well known dermatologist. He recognises *two* distinct series of changes in the skin which may follow vaccination, which have no connection with each other, and which differ in clinical significance. The first of these he designates *vaccinal local diseases of the skin;* these have their starting point at the place of vaccination itself, and thence may spread over the body; they include the ordinary inflammatory areola, erysipelas, and the eczemata. The second series consists of *general eruptions;* these appear very acutely, with or without febrile manifestations, and at a distance from the point of vaccination. It is this second class of cases, the *general* eruptions (excluding syphilis) with which my observations are concerned, as all the five cases I mean to relate to you were of this kind.

These eruptions, appearing *generally* over the surface, and not spreading by continuity from the vaccinated point, seem to be tolerably rare. Behrend gives six cases as his experience out of 300 vaccinations, and he adds a case which occurred in the practice of a friend. Two of these consisted of a macular erythema, resembling measles, and in both the eruption appeared on the eighth day. Others were of the nature of urticaria, coming on the day after vaccination. Two cases presented the characters of exudative erythema, beginning on the second day, spreading over the whole body, and showing the well known swelling and changes in colour. In another instance the eruption was vesicular, coming out the day after vaccination; while in the last case it was bullous, and appeared as early as the evening of vaccination. In the discussion which followed the reading of Behrend's paper, Dr. Hebra, of Vienna, said he had seen but one case of vaccinal eruption after the eighth day.*

---

* This paper is reprinted in full in the *Archives of Dermatology*, October, 1881, p. 383.

My own cases, five in number, may now be related. In three the lymph employed was calf lymph; in one it was only one remove from the calf; and in the last it was ordinary humanized lymph.

CASE I.—M. W., a healthy child, was vaccinated with calf lymph, obtained from London. All went well till the tenth day, when there appeared a plentiful crop of papules, scattered freely over the lower limbs, the lower part of the trunk, and on the arms. These never passed beyond the papular stage, but subsided, and had quite disappeared three days later.

CASE II.—This child was vaccinated with lymph taken from a child which had been successfully vaccinated with calf lymph eight days before. All went as usual till the eleventh day, when the mother discovered that numerous "blotches" had appeared on the child's skin. I found several large patches of typical *erythema exudativum*, large rings of vivid erythema, situated on a wide and tensely infiltrated base.

---

* More recently, this subject has been treated very exhaustively by Dr. P. A. Morrow, in the New York *Journal of Cutaneous and Venereal Diseases*, March, 1883. He groups vaccinal eruptions according to their elementary lesions, and agrees with Behrend in regarding them not as due to any specific action of the vaccine virus, but to "individual predisposition," and to "the presence in the blood of a morbific material, which may be the vaccine virus, or it may be the resorbed contents of the vaccine pustule," acting as simple irritants in circulation, and determining various eruptions, non-specific in their characters, presenting, in fact, a close analogy to the "drug exanthemata," or "incidental effects of drugs."

chiefly on the arms and thighs. The right hand and arm, and left foot and ankle, were very much swollen and deformed, the swelling not being sharply marked off from the surrounding normal tissues, but melting gradually into them. The redness was dusky, or even purplish in hue, shading off at the edges into a paler red. There was no pain, no irritation or itching, and the febrile disturbance was slight.

On the second day of the eruption the erythema had spread to both arms and legs, and some spots were found also on the body. The colour of the affected parts had become yellowish-red, and was disposed in distinct rings. The swelling was more marked and objectionable looking than ever. There was still little or no constitutional disturbance, and no swelling of the lymphatic glands.

On the third day of the eruption the colour was found to be fading in the parts in which it had first appeared—namely, on right arm and left leg; but on other parts it was still vividly out.

On the evening of the fourth day of the eruption the colour and swelling had quite gone, the parts formerly affected looking quite normal.

All this time the vaccinal crusting and areola had developed and subsided as usual—indeed, with rather less than the ordinary amount of inflammation and induration. Treatment consisted solely in the administration of sulphate of quinine and grey powder. With the same lymph, and on the same day, I had vaccinated two other children, and in neither case was there any departure from the ordinary march of the vaccinal phenomena.

CASE III.—This child, sister of the preceding, was vaccinated with calf lymph. Nothing unusual was noticed till the tenth day, when a vivid red eruption appeared over the whole body, including face and head, and consisting of small, slightly raised papules, closely set, and having a very striking resemblance to the eruption of measles. In the course of twenty-four hours most of this eruption had faded, the only parts in which it was visible being on the abdomen and back, under the binder. In other twenty-four hours no trace of the eruption could be seen.

CASE IV.—Vaccination was performed with calf lymph. There was nothing abnormal noted in the results of the operation till the ninth day, when an eruption appeared, having precisely the same characters and distribution as in last case. By the evening of next day it had faded away.

CASE V I hesitate to place in the same category with the others, mainly on account of the time that elapsed from date of vaccination till the appearance of the eruption—namely, four weeks; but I mention it as it possessed characters very much like those I have last described. The child was vaccinated with lymph taken by me from a healthy child in attendance after vaccination at the Faculty Hall. The vesicles passed through their usual stages, and inflammatory action had subsided, when, on the 28th day, after a day or two of indisposition, an eruption exactly like that of measles appeared on the child. There was no catarrh or any other sign which might indicate the presence of measles, and other members of the family who had not had measles were not affected in this way. The eruption consisted of bright red, raised, crescentic blotches, and was particularly abundant on the scalp and face, less abundant on neck and chest and upper part of arms, and absent from abdomen and lower extremities. On the third day it began to fade, and on the fifth it had disappeared entirely.

Besides this personal experience which I have detailed, I would ask you to notice some remarkable groups of cases of vaccinal eruptions which I find reported in certain American journals. With scarcely an exception they relate to cases in which bovine virus was used.

The first of these reports I can find is that of Dr. D. J. M'Millan.* He first refers to a communication made by Dr. Rice to the *Chicago Med. Journal and Examiner* for Feb., 1882, in which that gentleman states that one in ten of his vaccinated patients had high fever, and an eruption resembling rœtheln or German measles. Dr. M'Millan's experience included three cases of that kind, and in his patients the eruption was followed by desquamation. In another instance the eruption seems to have been erythema exudativum; and in a fifth case there appeared, on the eighth day, a scarlet rash, like that of scarlet fever, unaccompanied by sore throat. In all these cases bovine virus had been used.

Dr. A. H. Holt † records five almost exactly similar cases. In all of them vaccination was primary and performed with bovine virus. The eruption appeared from the eighth to the tenth day, was attended by considerable fever, lasted for two days, and then disappeared; it resembled measles, or rather German measles, from the description, being dusky-red, rough and distinctly elevated, and covering nearly the whole surface of the body and limbs; and scattered here and there were a few very minute vesicles. When it faded, a brownish stain was left in the skin.

---

* *Philadelphia Medical News*, 18th March, 1882.

† *Boston Medical and Surgical Journal*, 23rd March, 1882.

Dr. V. Y. Bowditch * records three such cases. In them the eruption appeared on the ninth day, and was succeeded by the same brown staining of the skin.

Dr. A. J. Lawbaugh † reports some still more striking statistics. Of 800 successful primary vaccinations, all with bovine virus, no fewer than 68 were attacked by an eruption, in all points identical with that already described. Also, of 13 successful primary vaccinations with humanized virus, one remove from the heifer, three had the very same eruption. Dr. Lawbaugh further states that many of his brother practitioners had met with similar cases.

Dr. M. Prince ‡ states that, as city vaccinator, he had vaccinated over 7,000 persons since the beginning of that year, and that eruptions accompanying successful vaccination were so frequently observed that he ceased to regard them either as unusual or accidental; but he was unable to say in what proportion of cases they occurred. They were usually papular or erythematous, the former being so common as to "cease to attract notice." He gives notes of two cases of severe urticaria accompanying vaccination, and one of very marked and widespread erythema, appearing on the sixth day.

Dr. J. L. Quin § writes that out of 130 vaccinations with vaccine virus, he had 2 presenting generalised eruptions. In both, the eruption appeared "within 24 hours after the arm began to show any considerable inflammation," presumably, therefore, about the eighth or ninth day; it took the form of very minute vesicles, not passing into pustules, closely set and scattered over the whole surface; these vesicles formed crusts in a few days, and when those fell off the usual brown stains were left.

As having a bearing on this matter, two other cases which I have noted may be mentioned. The first is one described by Dr. H. A. Martin ‖ as a "most rare, possibly unique, case of generalised eruption of vaccinia." A mother, with nursing infant, was re-vaccinated; child not vaccinated, as it suffered from extensive eczema. On 16th day the child became fretful and

---

* *Boston Medical and Surgical Journal*, 13th April, 1882.

† *Ibidem*, 20th April, 1882.

‡ *Ibidem*, 27th April, 1882.

§ *Ibidem*, 4th May, 1882.

‖ *Medical Record*, 15th April, 1882.

feverish, and on arms numerous little red pimples appeared. Three days later more had come out, to the number of 400, and had developed into circular umbilicated vesicles, especially abundant on the eczematous patches, and having the cellular structure of vaccine vesicles. The vesicles were small, desiccation was rapid, and no cicatrices were left, variations which the author ascribes to the distribution of the vaccinal process among so many different points, instead of the usual concentration of its force on two or three. Lymph taken from these vesicles at their height was inoculated on a calf successfully; from this calf four persons were vaccinated, all successfully.

The other case, an account of which was read before the Académie de Médecine, was Dr. Guéniot's.* A child suffering from eczema in the stage of retrogression was vaccinated at six points, all of which "took" well. On seventh day a multitude of small papules appeared on the arms, shoulders, and chest; these developed into vesicles, and ultimately into true vaccinal pustules, to the number of at least 300. Notwithstanding this occurrence, Dr. G. still counsels vaccination in eczematous children, but only two punctures should be made—one on each leg. In the discussion which followed, Professor Blot warned against vaccination in such cases. While Dr. Hervieux recommended it in eczematous children, as in others, he referred to some cases similar to Dr. Guéniot's, but thought, nevertheless, that generalised vaccinia is rare, as in 15,101 vaccinations he had practised for the Académie de Médecine, he had not once seen such an eruption, though doubtless many of these children must have been eczematous.

In conclusion, there are just two remarks I would offer on all this mass of experience I have quoted.

1. In nearly every instance I have mentioned in which spontaneous generalised eruptions followed vaccination, the lymph used was animal lymph, not humanised lymph. What does this indicate? That, as Dr. Cameron, M.P. once argued before this Society, the nearer the virus to its original source in the days of Jenner, the stronger it is, and the more efficient the protection it affords? Without venturing to give any opinion as to the greater efficacy of calf lymph vaccination as a prophylactic against small-pox—a matter which can only be settled on the basis of a wide statistical enquiry—it seems very clear that in animal lymph we have a more powerful material, one which more deeply and obviously affects the system than our ordinary humanized lymph, if the degree of

* *L' Union Médicale*, 18th May, 1882.

constitutional disturbance is to be taken as an index of the effectual working of the virus.

2. What is the nature of the pathological connection between vaccination and the occurrence of the spontaneous generalised eruptions? On the one hand we have the introduction of a specific virus into the system; on the other the occasional, or, if animal lymph be used, the frequent appearance of an eruption having on the whole tolerably constant characters. What relation subsists between the action of this virus and the cutaneous phenomena?

Behrend, in the paper which he read before the International Med. Congress at London, quotes Friedinger as a supporter of the view that the eruption is the result of the specific action of the vaccine matter on the organism in general, bearing the same relation to vaccine lymph that the eruptions of measles and scarlatina do to their specific contagia. Behrend himself endeavours to refute this view, and holds that such eruptions are not caused by any specific action of vaccine lymph, but that their occurrence proves nothing more than that there is a foreign material circulating in the blood; in the cases of eruption occurring shortly after the introduction of the lymph, this foreign material is the virus itself, and in the case of the later eruptions it must be matter absorbed from the pustules. And he bases his opinion—

(1.) On the polymorphous nature of vaccinal eruptions; an eruption which is at one time erythematous, at another papular, vesicular, bullous, or pustular, cannot, reasoning from analogy, be considered as due to a specific cause. We never hear of scarlet fever or measles presenting such varieties of eruption.

(2.) On the period at which the eruptions present themselves, some appearing within a day or so of vaccination, others about the eighth or ninth day; in other words, one group of eruptions appears during the incubation stage of the vaccine virus, the other at a time when the virus has already completed its action on the organism.

(3.) On the fact that we often observe exanthemata of an exactly similar character after the action of other agents, particularly drugs.

Personally I feel inclined to take up the former view: that the eruptions we have been considering are really specific and proper to the action of the vaccine virus, just as the measles eruption is to the measles virus, and so on. I believe Dr. Behrend to be partly right, however, in so far as he ascribes eruptions appearing soon after vaccination to the influence of the virus simply as a foreign material; and the very varying character of eruptions occurring at this period goes far to support this view. Behrend seems to me to have argued on a rather limited experience—only six cases. In view of the weight of testimony I have quoted to-night, however, I believe we shall not err in regarding these eruptions associated with the later period of the vaccinal action as quite specific in their character. They appear tolerably constantly from the eighth to the tenth day, and present a very close agreement in their general character and distribution. They are most usually described as consisting of fine dusky red or pale papules, distinctly raised, rough, occurring in patches over the whole surface, and having interspersed among them a few very small vesicles; crusting sometimes takes place, desquamation always, and the spots where the eruption had been retain a dark stain for a short time. The eruption remains out for about two days.

ON AURAL DIAGNOSIS.

BY JAMES PATTERSON CASSELLS, M.D.,
Aural Surgeon and Lecturer on Aural Surgery to the Hospital.

(*Lecture delivered in the Glasgow Hospital and Dispensary for the Diseases of the Ear, February, 1882.*)

THE PHYSICAL EXAMINATION OF THE EAR IN HEALTH AND IN DISEASE.

On the instruments that are required for the diagnosis of a case of ear disease.—Holding the belief that "simplicity, whether in nature or in art, is the highest perfection," I have always endeavoured to do my work with the simplest arrangement of instruments possible, and I have taught my pupils to do the same. For that reason, the instruments that I include in the following list are few in number, simple in construction, and inexpensive.* A more elaborately constructed set of instruments, or a more numerous array of them, would, I know well, prove embarrassing to the practitioner, without any equivalent compensating advantage. With these instruments, few and simple as they are, the simplest as well as the most serious cases of ear disease may be diagnosed.

* To be had at Messrs. Hilliard & Sons.

List of Aural Instruments:—

An aural mirror.
A set of aural specula. } Together constituting an auriscope.
A Siegle's pneumatic speculum.
A Politzer's or inflating bag and set of points.
An otoscope or auscultating tube.
A diapason (or tuning fork).
A set of Eustachian catheters.
An ordinary ear syringe with set of points.

The difficulty that attends the examination of an organ situated as the ear is, can only be fully appreciated by those who remember that its most important parts are quite hid from ocular inspection, and even incompletely within the reach of tactile investigation, differing in this respect from the eye, which not only admits of being readily examined, but assists us by its lenses to a more complete inspection of its interior.

Happily, however, this difficulty in the case of the ear is in great part obviated by a knowledge of the fact that these deeper lying and less accessible parts of the organ are comparatively seldom the seat of acute disease, and owing to their thorough protection are rarely injured; and although it be true that they can become the seat of acute disease, and can be injured also, these diseases or injuries seldom or never terminate fatally. On the other hand, it is a happy circumstance that those parts of the ear more accessible to our inspection, and more within the reach of our treatment, are the parts of the organ most liable to acute disease, as well as injury, which are not unfrequently followed by fatal results.

But, fortunately, the difficulty of examining the more hidden parts, though not removed, is in a measure lessened by the improved and newer methods of examining the organ now at the disposal of the practitioner.

For, till a period comparatively recent in the history of the progress of otology, the surgeon who wished to examine the interior of the organ was compelled to the employment of direct light alone. It was in this way that many awkward and even fatal mistakes were made by men even of the highest repute; nor could it well be otherwise under such disadvantageous circumstances.

Now, however, as I have said, the difficulties of earlier times have been in some measure overcome by the surgeons of to-day, helped by newer methods of investigation, and by a fuller and more perfect knowledge of the diseases of the ear. With the concave reflecting mirror and the aural speculum we can at all times, and in any position, examine with the greatest ease, both to our patient and to ourselves, the interior of the ear. Without these instruments it is not too much to affirm that the *practice* of aural surgery could not exist as it now does.

For this reason it becomes difficult to over-estimate the value of the obligation that we owe to Professor von Troeltsch of Würzburg, and to the late Sir Wm. Wilde, of Dublin, for their disinterested contributions and labours in this respect. For to the former gentleman belongs the honour of being the first person to introduce to general use the concave mirror as a means of illuminating the interior of the ear, while it adds to the well merited renown of the last named distinguished man to be able to say that he first introduced the conical ear speculum to the notice of the profession in this country.

Premising that direct light is only employed by us now in the ocular and tactile examination of the external ear region, and that reflected light is always and solely used in the inspection of all the parts of the organ lying within the orifice of the external auditory canal, we now proceed to—

The examination of the patient.—Having placed the patient near to a window with a northern aspect, and through which a flood of clear daylight is streaming, turn the affected ear towards the window. Standing on one side, so as not to obstruct the perfect illumination of the parts, the surgeon proceeds to examine all that part of the lateral aspect of the head which I have ventured to call—

The external ear region.—It comprises all those parts in proximity to and including the auricle. Two lines drawn obliquely to the perpendicular and across each other at right angles, having the centre of the auricle as the point at which they cross each other, divide this region into four quadrants—viz., a superior auricular and a parotid region, a tragal and a mastoid region or quadrant respectively. The advantage of so mapping out this region is, that one can easily localise the site of any abnormality, or indicate to another with precision the part to which a remedy is to be directed or applied.

Now, let the surgeon examine, by eye and hand, if there be any abnormality in the tissues of this region. Is the auricle entire and normal in form and in position with relation to the side of the head, or is it the seat of tumours, excoriations, ulcerations, &c., and is it pressed flat upon the side of the head, and from what causes have these arisen? Then, note the state of the tissues immediately surrounding the auricle; are they natural in colour and density, or are they red and swollen, or "boggy" as well as painful to the touch, and the site of these; are there any sinuses over the mastoid, and their condition and history? Finally, examine as to the state of the orifice of the meatus, if it be normal or closed, and the nature and history of the obstructing cause.

Having thus completed the inspection of the external ear region, it becomes necessary in most cases, before commencing the examination of the parts lying within the orifice of the meatus, to *test the hearing distance* of the patient. This is done in two ways—by the tick-tack of an ordinary watch, and by the tones of the human voice, the respective values of which, I shall point out by and bye. Having ascertained by experiment what is the average distance at which your own watch is heard by several people, who are *perfect* in the sense of hearing, you become provided with the best and most convenient acoumeter—a name originally given to an instrument devised by Wolcke (afterwards improved by Itard) to measure the hearing distance of those who were either deaf or dull.

I have just said that one's watch, for which an average hearing distance has been established, as above directed, is the best and most convenient acoumeter; and it is so, if there be any force in the fact that the watch has held, and still holds, its place in the estimation of practitioners, and against the seductive influences of numberless inventions. Nor need this excite surprise if we consider for a moment the manifold advantages that a watch has over the questionable superiority of a specially devised instrument, whose chief, but not sole defect is, that it is not portable, or so portable, as the ordinary timepiece.*

The ordinary watch, as a measurer of the hearing distance is, however, only a correct test of the limit of the perception of the ear to one, or, at most, two tones; in fact, to the tones emitted by the motion of the wheels of the watch, and which have, in imitation of the sound thus caused, received the name of tick-tack. It can, however, by an ingenious arrangement, be made to become a standard of comparison to other surgeons, who, having subsequent charge of a case, may desire to know the progress that it is making under their own treatment.

This is done by a method (first suggested by Proud of New York), and which I have used in my own practice for several years. This method of testing the hearing distance is somewhat similar to that used in testing the sense of sight by ophthalmic surgeons, by Snellen's types, because, as you know, watches are as various and variable as are the faces of mankind. Nevertheless, an approach to accuracy may be obtained by this method, which I am about to describe more fully, sufficiently precise for all the exigencies of practice. This method is as follows:—Convert the distance at which the tick-tack of your watch is heard by the patient and the affected ear—the opposite ear being closed meanwhile—into a fraction, of which the numerator indicates the distance at which the watch tick is heard clearly by the patient, and the denominator is the normal average hearing distance, at which your watch is heard by a healthy ear. Thus, if the distance be recorded in inches, the following formulæ, R. $\frac{0}{72}$, L. $\frac{72}{72}$; or, R. $\frac{12}{72}$, L. $\frac{22}{72}$, mean as follows:—In the first example, the watch, which is heard by a healthy ear at a normal hearing distance of 72 inches, is not heard at all when laid on the right ear, as is indicated by the cypher or zero symbol. On the left ear, the figures indicate that the watch is heard quite normally, and so on, for the other examples, which are self-intelligible after this explanation.*

---

* I need not say that these remarks do not apply to Professor Politzer's well known universal acoumeter.

Now, let us suppose that a patient, who has been under the care of a practitioner for some time, is sent to another one at a distance for further treatment, and that this surgeon sends with the patient a history of the case, which, among other things, states that the hearing distance was on a certain date 20 inches, and which the surgeon represents by the formula R. $\frac{20}{72}$. How is the other practitioner to estimate this information so as to be able to establish a comparison between the hearing distance at two different periods, and made with different watches? This practitioner finds that, on testing the patient with his watch, the hearing is to be represented by the formula R. $\frac{4}{40}$—that is, that the hearing distance is four inches to a watch whose average normal hearing distance is 40 inches. The second practitioner, having now two formulæ before him, has only to do a sum in proportion to find the actual difference, if there be any, between the hearing distances of the patient at the two different, and, it may be, widely separate periods of time, and which hearing distances are represented by the respective formula—thus, as 72 : 40 : : 20 : 4. In this way a perfectly accurate decision could be arrived at in respect to the present hearing distance of the patient, and by comparison with the past state, one could estimate the nature of the progress of the case. All this is, however, on the assumption that the circumstances in which each of these testings were conducted are the same as regards the temperature of the surrounding air, the presence or absence of currents, and the presence or absence of conflicting noises. Upon the whole, although absolute accuracy is hardly attainable, as near an approach is made to it as is necessary in the interests of the case.

---

* I may here mention that the letter C in one of these formulæ indicates that the watch is only heard when placed in contact with the patient's ear.

In testing the hearing distance with one's watch, the following is the mode of procedure:—Cause the patient to close both eyes (this is indispensable with children), and with the finger-tip to close the ear not under examination. The surgeon, standing in front of the patient with watch in hand, at first holds it at arm's length from the effected ear, and, supposing that it is not heard at this distance, gradually makes the watch to approach to the side of the patient's head, until its tick-tack is distinctly heard—this is called testing the hearing to the ærial tones of the watch. Meanwhile, take care that the course of the watch towards the side of the patient's head is in the line of the axis of the auditory canal, otherwise the results will be vitiated by apparently unexpected, although not unexplainable errors—the line of the auditory axis being the posterior limit of the field of maximum intensity of hearing power. Next, in the same way, test the hearing distance of the patient's unaffected ear, and having done so, compare this with that of the affected one, and also both, with your normal standard, whatever that may be. In this way, you rapidly arrive at a knowledge of the degree of the patient's hearing, or, in other words, of the amount of the defect in the affected ear or ears. But, remember always, that all this is a testing of the patient's perception of the ærial tones of the watch's tick-tack only. For clinical experience shows that the tick-tack of the watch is only a criterion of the patient's hearing for its own or similar ærial tones, and not at all a measure of the degree of perception of the tones of the human voice. For I have over and over again had patients under treatment for deafness, whose defective hearing remained almost unchanged in regard to the perception of these ærial tones, but who, nevertheless, became quite, or almost quite, perfect in their perception of the human voice, and *vice versa*; and further, from some experiments that I made on a healthy ear on this very point, I found that while the ordinary sounds, such as street noises and articulate speech, became gradually obscured and at last altogether shut out, the tick-tack of an ordinary watch, suspended in the same room, at a distance of about 10 feet from the person experimented on, was heard distinctly during all the experiments, and even clearer at the end of it, at which time all other noises, even the crackling of the fire in the room-grate, were quite unheard; indeed, at this stage in the experiment, the tones of the watch were said to be more metallic, and much clearer than at ordinary times.*

Inasmuch as the object of a patient who consults you, and is simply deaf, and not a sufferer from some acute and painful ear disease, is to recover the power of hearing the tones of articulate speech, and not only to hear the tones of the watch or any other mechanical acoumeter, it becomes necessary at the outset, and before treatment is begun at all in the case, to arrive at an approximate estimate of the amount of the defect that there is to the perception of these tones. It is, however, unnecessary for me to occupy time in describing the mode of doing so; every intelligent practitioner, in testing the patient's hearing power by the tones of the spoken voice, will be able to devise a convenient mode of procedure to suit his own needs.

In relation to this mode of testing the hearing, I have only to call your attention to the relative values of the monotones and polytones of the voice in the articulation of monosyllables and polysyllables respectively. (See Oscar Wolf, *Sprache und Ohr*).

In conclusion, as to testing the hearing of a person who is defective in this respect, I have to say that I regard distinctly spoken sentences, during the utterance of which the patient faces the speaker with shut eyes, and is desired to listen, as the justest as well as the most satisfactory mode of testing, either as to the amount of the original defect, or subsequently and after treatment, as to the degree of improvement that has taken place in the patient's case. Never forget, however, that hearing is a relative quality, and that the slight deafness of one person may to another seem to be fair hearing, or on the other hand, might even be as total deafness to a third person; the lesson from which observation is this—in every case do your best to satisfy your patient. For it matters little to such an one to be told that, judged by your standards of comparison, he hears better to-day than he did some days previously, if the patient be quite satisfied, from his own experience, that he does not hear the voices of his associates one whit better. For, I repeat, the tones of the human voice, blending themselves in melodious articulate speech, are the truest, because the most natural, test of the state of the patient's hearing.

---

*I had on one occasion a patient—a female—who suffered from chronic catarrh of the tympana and tubes, who complained of a constant ticking in the left ear, which she likened to the tick of a watch. It was not always present, however; but on one occasion, on testing her hearing, I was surprised to find a sudden, and, I may add, quite unexpected gain in that ear to the hearing of the watch. I expressed my delight at the great and seemingly marvellous improvement; but the patient knew better, and, as she declared that the ticking was in her ear, I placed my auricle in close contact with hers, and heard sounds in the woman's ear as distinct and clear as the tick-tack of an old fashioned verge watch, from which it was quite impossible for me to distinguish it. On examining the ear while this ticking noise was going on, I could not discover any apparent cause for this so remarkable phenomenon; but it cannot be unique, and I only therefore mention it here to you in order to put you on your guard, so as not to be deceived, as I nearly was, and would have been, but for the intelligent acuteness of the patient. I shall, probably, refer to this and other anomalous forms of tinnitus, or subjective ear noises, farther on. Dr. Fitzgerald, of Dublin, informs me that he found this ticking in the ears to be associated with spasm of the palate muscles.

Not only, however, have we to test the patient's hearing as judged by this perception of the aerial tones of the watch and voice; but it is necessary, in all cases, to investigate into the degree of perception of the tones of a vibrating body when transmitted directly to the auditory nerve.

Testing the degree of tone perception by the tuning fork or diapason is effected in the following manner:—Place a tuning fork in vibration on the vertex (c = 512 and clamped). If the ground tone of the instrument is heard equally well in both ears, the meatuses being open, this is = *normal tone perception.*

If the meatuses are closed, and the vibrations of the instrument are heard equally in both ears = normal tone perception also.

If the above tuning fork be converted into a diapason by shifting the clamps, it serves to test tone perception to the full extent, the octave.

If it is heard under the above conditions = normal tone perception for all tones of the octave.

If it is heard from the vertex better in one ear = some defect in the sound transmitting apparatus of the ear is indicated.

If not heard at all from any part the head = total loss of sound perception—total deafness.

Obituary.

PETER STEWART, M.D.

Dr. Peter Stewart died on 10th May, 1883, after a long and painful illness. He was in his 70th year. By his death, Glasgow has lost one of her best known and most highly respected medical practitioners; a man of skill and worth; a physician in good repute among his professional brethren, and beloved by his patients.

He was born in Greenock on 16th November, 1813. His early boyhood was spent in the Highlands, and he received his education in the Grammar School of Paisley. Subsequently he studied medicine in the University of Glasgow, where he took the degree of M.D. in 1845. After obtaining his degree, Dr. Stewart repaired to Paris, where he pursued his medical studies for a period of two years. On returning to this country he commenced practice in Glasgow; but his health gave way, and he went as "assistant" to England, where horse exercise and new surroundings restored his vigour. Once more returning to Glasgow, Dr. Stewart resumed his professional labours on the South Side, and here he succeeded in establishing an extensive practice. He worked hard, sparing no personal exertion or trouble in the discharge of his duty to his patients, a duty which was to him truly sacred.

In a profession which is acknowledged to be self-sacrificing, he was distinguished by a conscientious devotion to the interests of those under his care. In 1868 he removed to the north side of the river, taking up residence at Albany Place, Sauchiehall Street, which has since continued to be his home. His old patients on the South Side still clung eagerly to him, and, for their convenience, he kept consulting rooms at Bridge Street, where he regularly attended for consultation. At great personal inconvenience and loss he faithfully attended his old and, in many cases, poor patients; they insisted on having him, and he would not break the old bond.

Dr. Stewart now entered on a phase of his life in which he certainly overtaxed his energies. The first distinct threatening of a breakdown was in 1876, when he contracted a severe pleurodynia by lifting a helpless patient into bed. He still continued to work hard—no call was neglected, his poor patients still having the strongest claim on his sympathies and his services. Advancing years did not damp his ardour, and in spite of the remonstrances of his friends, he continued to meet all engagements.

About three years ago a decided failure in health took place. There occurred "faintings," together with a marked change of manner and indications of commencing laryngeal mischief. For the latter disorder resort was made to more genial climates, and eminent authorities were consulted. But the evil slowly and steadily progressed. Again returned to his residence in Glasgow, Dr. Stewart was confined to bed, and the suspicion of malignant disease of the larynx was confirmed.

He bore his sufferings with great fortitude. Even in his misery his mind was busy in devising means for the good of others. When suffocation seemed imminent, tracheotomy was performed, the result being decided relief.

For nearly three weeks his condition remained practically unchanged (with the exception of increasing mental aberration), but then rapid sinking took place, and he died quietly at half-past one o'clock on the morning of 10th May. *Post-mortem* examination of the larynx revealed complete disorganisation, due to malignant disease.

During his illness he had the comfort and solace of the devoted and unwearied attention of his wife, daughter, and son-in-law; and after his return to Glasgow, old friends, lay and medical, flocked to his side, all eager to render any service in their power.

Dr. Stewart was elected a Fellow of the Faculty of Physicians and Surgeons in 1858, and in 1854-5, 1878-9, he was President of the Glasgow Southern Medical Society.

He was for some time one of the Managers of the Glasgow Royal Infirmary, in which capacity he exerted his influence for the better recognition of the services of the medical staff.

He was very fond of travel, and in company with his wife and daughter, visited most of the countries of Europe, as well as various parts of America, Australia, and New Zealand.

In all the relations of life he was honourable and exemplary, and although not ostentatiously religious, he was a sincere Christian.

He has now gone to rest, but his memory will live in many grateful hearts.

REVIEWS.

A Manual of Pathology. By JOSEPH COATS, M.D., with Three Hundred and Thirty-nine Illustrations. London: Longmans, Green & Co. 1883.

THE work before us forms a very handsome volume of over 900 pages, and, in its general appearance, it reflects great credit on printer and publisher. It has been printed in Glasgow at the University Press, and both in the letterpress and the numerous illustrations the highest excellence has been attained. The bulk of the illustrations are original, and the engraving has been done in Glasgow by Mr. Stephen Miller. To these also, nothing but praise can be awarded, and the merit is the greater, as microscopic illustrations are a severe test of the engraver's art, because their objects are outside his experience. In this regard the draughtsman shares at least equally with the engraver in the merits of the production.

It will not be expected, considering the relations of the author to this *Journal*, that the contents of the book will receive the same kind of criticism here as may be looked for in other *Journals.* It may be useful to our readers, however, to give some kind of summary indication of the scope and contents of the book.

The title of the work is *A Manual of Pathology*, and as the name is a somewhat general one, it will be found that the text does not confine itself to the mere details of Pathological Anatomy, but that the whole phenomena are brought under review, and an attempt is made to construct a complete picture of each disease. In the words of the Preface, "It will be acknowledged that the whole aim of pathological anatomy is to elucidate the actual vital processes of disease, and it has been felt by the author in lecturing on Pathology that the subject gains in interest and intelligibility when the etiology, anatomy and general pathology are all taken into account in forming a general conception of each morbid condition." No doubt this plan has caused an increase in the size of the volume, while it has perhaps made it less systematically complete than if pathological anatomy alone had been taken up; but it has probably rendered it much more readable, and more accessible to the general members of the profession.

The general body of the work is divided into two parts, the first including General Diseases; and the second Diseases of the Special Organs and Systems.

In the FIRST PART, the AFFECTIONS OF THE CIRCULATION AND OF THE BLOOD are first taken up. After a preliminary discussion of the arrangements concerned in the regulation of the distribution of the blood, the subjects of Hyperæmia and Anæmia are entered upon. GENERAL HYPERÆMIA is defined as "the condition in which the total quantity of blood in the body is in excess," and is regarded as equivalent to PLETHORA. In connection with this subject that of TRANSFUSION is incidentally introduced, as by this operation a condition of ARTIFICIAL PLETHORA may be induced in animals. By reference to experiments, however, it is shown that any excess of blood introduced into the system is disposed of more or less rapidly. The subject is concluded in the following words, "Having now seen that, by transfusion, an artificial plethora may be induced in animals, we may be prepared to admit the possibility of its existence in the human subject. It will presumably occur when the blood-forming organs are unduly active, and will manifest itself in an over-fulness of the capillaries and veins throughout the body, but especially in those of the abdominal viscera. Persons of vigorous digestion and active habits have often a florid appearance, as if the vessels, of the skin at least, were overfilled. The excess of blood is used, to a considerable extent, in the formation of fat, and we know that the subcutaneous adipose tissue and that of the abdomen are often much increased. But the observations mentioned above show that any excess of blood is disposed of with considerable rapidity, and we may infer that in the human subject a moderate tendency to plethora will be overcome. It will develop when the formation of blood keeps in advance of its destruction by the arrangements provided for that purpose."

Following on the discussion of Hyperæmia and Anæmia, the important subjects of THROMBOSIS and EMBOLISM are taken up. In regard to the latter, the different results of obstruction in arteries with or without anastomosing connections, are detailed, and the pathology of the hæmorrhagic infarction described, chiefly on the basis of Cohnheim's observations. This division concludes with a reference to embolism by oil and by air. In regard to the former the following observations are made.

"Oil or fluid fat not unfrequently gets into the blood-vessels. It does so where, by fracture of a bone or injury to the subcutaneous adipose tissue, the fat cells in the bone marrow, or adipose tissue are broken up so as to set free the oil, and, at the same time, the vessels and especially the veins are laid open so as to absorb the oil. Oil also occurs in the blood in cases of diabetes, and, as in Fig. 10, may produce embolism. The oil is carried, of course, to the right heart, and on into the lungs where it sticks in the capillaries and the smallest arteries. If the capillaries are obstructed at considerable intervals then no evil results, the anastomosis being so free that no serious disturbance occurs, and the oil itself does not irritate. But, if a considerable number of small arteries near each other be obstructed or even a large number of capillaries, we may have a condition resembling the hæmorrhagic infarction. It has been found on experiment that abundant fat embolisms in the ear of a rabbit, by obstructing numerous capillaries, may lead to necrosis. In some cases the oil may, to some extent, pass through the lung capillaries, and it is found mostly in the Malpighian tufts of the kidney, one or two loops being here and there filled. There are a few rare cases on record in which extensive embolism has occurred in the smallest vessels of the brain, conjunctiva, skin, muscles, heart, intestines, liver, and kidneys, and has apparently been the cause of death."

The subjects of HÆMORRHAGE, OEDEMA and DROPSY, LEUKÆMIA, MELANÆMIA, URÆMIA, and DIABETES MELLITUS complete the section on Diseases of the circulation of the blood.

The next great subject is the important one of INFLAMMATION, and here the observations of Lister receive more consideration than they have sometimes obtained. The principal phenomena of inflammation are described under three heads—namely, the state of the vessels, the inflammatory exudation, and the changes in the tissues, including new formation. In connection with the last mentioned, the granulating wound is taken as the type of the inflammatory new formation. We may here quote the observations made in regard to the action of the tissues on dead tissue or foreign bodies—

"Of considerable importance is the NEW-FORMATION of tissue AROUND FOREIGN BODIES. If a foreign body be introduced among the living tissues, if not very virulent in its own nature, it sets up a mild chronic inflammation, with the result of producing a vascular rudimentary tissue like granulations. If the foreign body be permeable by this tissue then the granulations will grow into it, and, as it were, devour it, replacing it first by their own rudimentary tissue, which afterwards gives place to connective tissue. As this connective tissue is comparatively small in bulk, and tends to contract more and more, the result of the whole process is an absorption of the foreign substance and the gradual disappearance of all trace both of it and of the tissue which has replaced it. But if the foreign body is not permeable, then the inflammation results in the production of a layer of connective tissue around it, and so the body becomes encapsuled.

"Many instances of this might be given. If a piece of dead animal tissue be introduced into the body, as, for instance, a piece of liver previously hardened in chromic acid solution, or a piece of prepared catgut used to ligature a vessel, then the dead tissue is first replaced by rudimentary tissue which gradually shrinks away. If a piece of the tissue of the body dies, and if severe inflammation is kept off, then its replacement by rudimentary tissue and absorption occur. In fractures of bones it often happens that a piece is entirely separated and dies. Such a piece of bone may lie exposed in the wound in a compound fracture, and it has frequently been seen how it has been eaten into by the granulations and absorbed by them."

The subject of RETROGRADE METAMORPHOSIS follows on that of inflammation, and this is followed by HYPERTROPHY, REPAIR and REGENERATION. In regard to the repair of injuries and restoration of lost parts, the following remarks may be quoted—

"In man, and in the vertebrata in general, the long course of development seems largely to exhaust the reproductive power of the body, and, in the adult state at least, the power of restoration of lost parts is very small, and the processes concerned are almost as much related to growth of tissue as to development. In the embryonic state it is probable that the power of restoring lost parts is much greater than in the adult. Some children are born with a short arm, at the extremity of which are imperfectly developed fingers; it is probable that in these cases amputation of the arm has occurred in utero, and an attempt at restoration has followed.

"In the adult it may be said that restoration of lost structures is almost confined to the blood and the epithelial and connective tissues. Along with the connective tissues we have, of course, blood-vessels which are readily reproduced, and we may also, to a limited extent, include nerve fibres, which, as we shall see afterwards, are sometimes restored.

"While, therefore, the absolute restoration of complete and considerable portions of the body is scarcely possible in man, yet the body is by no means unprovided with powers by which INJURIES ARE REPAIRED and loss of structures is made good. It may be said that the higher and more complex animals are endowed with greater ability to protect themselves from injury, and that their tissues possess the necessary powers of restoration in the case of those injuries to which they are specially liable. In the various processes here to be considered, it will be seen that what may be called the definite intention to attain a certain result which is shown in the process of development is distinctly visible, and that the tissues have a remarkable power of meeting adverse conditions."

The subject of INFECTIVE TUMOURS, including, among others, syphilis and tuberculosis, is a very important one. In regard to syphilis, it is pointed out that it is due to a virus. The primary lesion is the effect of the local application of the virus. In the secondary stage, the virus has reached the blood, the blood and secretions are contagious, and the lesions, which are chiefly inflammatory, are symmetrical. The tertiary stage is characterised by the occurrence of gummata, which are non-symmetrical tumours of a particular structure. The relations of the gumma to the virus are thus referred to—

"The virus no longer exists in the blood, and in accordance with this the lesions are characteristically unsymmetrical. The most probable supposition is that, after the close of the secondary stage some of the virus has remained lying in a particular spot. Perhaps a small gumma has formed, and the virus has lain in it quiescent but still surviving. It may be waked up by some accidental circumstance in the life of the patient, at any period afterwards, perhaps as long as twenty years. The virus propagates itself, but its effects are local. It may produce a tumour of large size, but it does not pass into the blood, and does not produce the lesions of the secondary stage."

The next subject taken up is the important one of TUBERCULOSIS. This subject is treated of in the view that it also is due to a virus. In General Tuberculosis the virus is in the blood, and many organs are affected, the condition being comparable to an acute fever or to the secondary stage of syphilis. In local tuberculosis, the virus remains confined to the tissues, producing such conditions as phthisis pulmonalis, renal phthisis, tubercular peritonitis, &c. The nature of the disease is further illustrated by reference to the artificial production of tuberculosis in animals, and in regard to the nature of the virus, the well known observations of Koch are referred to. Figure 56 is a representation of the tubercular bacillus.

The next great division of the work is TUMOURS or MORBID GROWTHS. In regard to the origin of such tumours, the view of Cohnheim is referred to, according to which growths may arise by little pieces of embryonic tissue having been left over from the embryonic state, and retaining their power of growth by reason of this. It is pointed out that while this view may apply to some tumours it does not meet the case of all, and especially that it does not apply to Cancers.

"We shall afterwards see that these tumours do not grow from a single piece of tissue, but that in a considerable and extending area, the existing epithelial structures take on an unusual growth. At the marginal parts of an epithelioma of the tongue, for instance, it can nearly always be seen how the normal epithelium is extending and penetrating inwards; and again, a cancer of the kidney appears to arise and advance by transformation of the existing tissue of the organ. It appears, therefore, that in this case the tumour arises from the existing normal structures, and that we must look for some abnormal impulse inducing it to grow outrageously."

In the description of the various forms of tumours considerable advantage is taken of the woodcuts in Virchow's classical work on tumours, and the author acknowledges the kindness of Professor Virchow in lending the necessary cuts.

The part of the book relating to PARASITES will be looked to with some interest, especially at the present time when so much attention is being paid to micro-organisms. These organisms, being the lowliest in the scale of life are first taken up here. Of these the commonest forms, which occur in decomposing matter are first referred to. This naturally leads to the consideration of septic diseases, including septicæmia and pyæmia. In regard to the former, the distinction is carefully drawn between its two forms. In the words of the author—

"In considering the phenomena of septicæmia we have two possible conditions to take into account. If putrid material be introduced into the blood it may produce symptoms due to the mere poisonous nature of the products of decomposition—namely, the volatile oils which give their odour of putrescence and other chemical principles. But on the other hand it may give rise to symptoms due to the bacteria which may live on in the blood and produce mischief there, multiplying as such organisms do with greater or lesser rapidity. We may thus distinguish a septicæmia due to chemical poisoning, and another due directly to the action of the organisms. We shall first take up the former of these, and it will be convenient to refer to it as *putrid or septic poisoning* as distinguished from *putrid or septic infection*. (German writers use the expression intoxication as equivalent—it is so etymologically—to poisoning, but this use is often rather confusing to English readers, especially when met with in translations.)"

The whole of this section is abundantly illustrated by frequent figures, some of them being copied from Koch's works.

Turning to PART SECOND, which deals with diseases of the special organs and systems, the first subject taken up is that of DISEASES OF THE ORGANS OF CIRCULATION. Under this head are considered the various diseases of the heart, blood-vessels lymphatic system, spleen and bone-marrow.

Next come the DISEASES OF THE NERVOUS SYSTEM, divided into these of (*a*) peripheral nerves, (*b*) spinal cord and medulla oblongata, (*c*) encephalon, and (*d*) membranes and cavities of the brain and cord. The section has an introduction in which the general plan of the nervous system is described, and the various orders of centres referred to. Each division of the subject has also an introductory paragraph, chiefly anatomical and physiological, the importance of connecting pathological lesions with their anatomical site being recognised. The manner in which this is done may be gathered from the following quotation from the introduction to the section on diseases of the cord:—

"THE WHITE SUBSTANCE, consisting of nerve fibres, forms connections in the first place with the various orders of centres in the cord itself and in the second place with the higher centres above the cord, in the brain. We may thus distinguish two sets of nerve fibres, one forming connections within the cord and medulla and the other forming communications between the cord and the cerebellum and cerebrum. These two sets of fibres will be divisible again into afferent and efferent.

"It is a fact of very peculiar interest, that the two sets of fibres distinguished above seem to be developed not only separately but at different periods, and so the aid of embryology has been sought to enable us to distinguish between them. The fibres which connect the different parts of the cord and medulla with each other may be regarded as the primary or fundamental ones, and it is found that they are the first formed, while those forming higher connections are of subsequent development. As nerve fibres are first developed without the medullary sheath, and recently formed tracts are therefore much paler than those which have acquired it, we have in this a means of distinguishing the fundamental from the secondary. What follows will be understood by reference to Fig. 194. which represents a transverse section of the cervical cord in the fœtus of nine months.

"The first developed fibres immediately surround the grey matter, and are called the ANTERIOR and POSTERIOR ROOT-ZONES. As they form communications between one part of the grey matter and another, the fibres are comparatively short and their number is generally in proportion to the amount of grey matter, or at least of ganglion cells in the horns. These fibres therefore do not diminish from above downwards as do the others.

"Of the fibres forming communications between the brain and cord, the best known are those which convey the motor impulses from the brain to the cord. These form the PYRAMIDAL TRACT. We shall afterwards trace them from the cortex of the brain downwards, but at present we take them up at the medulla oblongata. Here they form the anterior pyramids and most of the fibres decussate so that in the cord they occupy the opposite side to that which they have in the brain. Some of them, however, do not decussate, but remain in the anterior parts of the cord, forming a small band on either side of the anterior longitudinal fissure, the COLUMNS OF TÜRCK (*T* in figure). The great mass of the fibres, having decussated, pass to the lateral column of the cord, where they occupy a definite position in its posterior parts (*pt* in figure). The fibres in both these positions diminish in number from above downwards, as they pass into the grey substance of the cord at successive levels in order to connect with the centres in the anterior cornua."

Under the Inflammations of the Cord care is taken to distinguish between transverse and systematic myelitis. In the latter the inflammation follows special physiological systems, and this fact is referred to as being connected with that noted in the quotation given above, according to which the various sets of fibres and ganglion cells have a separate development.

The DISEASES OF THE ORGANS OF RESPIRATION follow next, being divided into those of (*a*) the air passages, (*b*) the lungs, and (*c*) the pleura. In regard to the pathology of croup and diphtheria the following is the concluding paragraph:—

"We have seen that in the larynx diphtheria produces an inflammation in which, after the shedding of the epithelium, a fibrinous exudation occurs. It is asserted by some that the diphtheritic poison is the only agent capable of producing this form of inflammation. Looking at the matter from a purely pathological point of view apart from clinical experience, it certainly seems possible that other irritants may produce similar results. Croup has been produced artificially in rabbits by the injection of ammonia into the trachea (*Weigert*). In these cases the irritant first kills the epithelium, and then fibrine is deposited. If croup occurs in man apart from diphtheria, the irritant must be strong enough to destroy the surface epithelium. Apart from the action of minute organisms, such an irritant must be of rare occurrence, but the possibility of its existence is not to be denied."

Nearly 20 pages are taken up with the pathology of phthisis pulmonalis. One of the most interesting points is cursorily discussed in the following paragraphs:—

"We have still to consider the CONDITIONS of the lungs WHICH PREDISPOSE to the occurrence of phthisis, and the path by which the tubercular virus finds access to the lungs. All authors seem agreed that phthisis is in many cases hereditary. That is to say, persons are born with a condition of lungs which renders them peculiarly susceptible to the changes which are to be described below. Taking the view that these changes are due to the tubercular virus it seems that persons are born with constitutions peculiarly incapable of resisting the action of the virus. It need hardly be said that this is perfectly consistent with what we know concerning other diseases. There are families in which diphtheria and scarlet fever make the most serious ravages, and there are others which they never attack. There are persons who are attacked by every form of fever in turn, there are others equally exposed who take none. But, besides an inherited liability, there is an acquired tendency to phthisis. This occurs principally in persons placed in circumstances in which the general health is reduced, and where especially the respiratory functions do not get justice. Persons living in close dwellings, especially when at their work, in factories and otherwise, they are in the habit of breathing vitiated air, in which, it may be, finely divided dust is abundantly suspended, frequently acquire a tendency to phthisis although originally free from it.

"We have now to inquire as to the PATH OF ENTRANCE of the irritant to the lungs. In the study of the lesions met with we shall find that they all start at the finer bronchi. A catarrh of the finest bronchial tubes, usually occurring in a number of these simultaneously, is the starting point of a variety of lesions, which, however, for a considerable time remain related to the bronchi in their distribution. This is a sufficient indication that the agent finds access to the lungs by the inspired air."

The DISEASES OF THE ALIMENTARY CANAL form a somewhat long chapter. At the end of the section devoted to the diseases of the intestines, the following remarks occur under Obstruction to the calibre of the intestine:—

"This condition has been incidentally mentioned as occasioned by several of the lesions already described. It may be well here to sum up the various forms of intestinal obstruction and to describe the effects produced when the calibre of the intestine is interrupted.

"Next to hernia, the most frequent cause of obstruction is the contraction of cicatrices resulting from ulceration, and in the great majority of cases the ulcers are malignant, that is to say, they arise by the breaking down of cancers. It is to be remembered that in contractions of the calibre such as these the direct cause of the final obstruction may be the folding of the mucous membrane above the stricture, this acting like a valve to the narrowed part. Tumours also obstruct sometimes by their bulk. Similarly gall stones, masses of fæces, collections of round worms may occlude the calibre. Invagination is the commonest cause in children. There is also twisting or torsion. Lastly, we have obstruction by incarceration, the intestine passing into a position where a loop is caught at the neck and strangled. Hernias form the commonest examples of this kind, but there are a number of cases in which the intestine slips under a bridge or ligament and gets, as it were, pinioned and compressed.

"This last cause of obstruction occurs so obviously to the mind that its frequency is apt to be thought much greater than it is. In reality it forms a small proportion of the entire cases." . . . .

The DISEASES OF THE LIVER, PANCREAS AND PERITONEUM, are grouped together in one chapter. The subject of hypertrophy of the liver is treated of in the following paragraphs:—

"When considerable portions of the liver are destroyed, there is liable to be a COMPENSATORY HYPERTROPHY of remaining parts. The loss of liver substance may be congenital, as in a case observed by the author in which, probably from some injury early in fœtal life, the right kidney and the greater part of the right lobe of the liver were awanting. In that case the left lobe was greatly increased in size, and the liver as a whole was of the normal weight. There may be a similar compensatory hypertrophy from destruction of liver tissue in after-life. The destruction may be by pressure of hydatid cysts, syphilitic gummata, and cicatrices, or even by cirrhosis. The region of the atrophy will determine that of the hypertrophy, but sometimes the left lobe or the lobus Spigelii undergoes great enlargement.

"Hypertrophy of the liver has been described as occurring in DIABETES. Undoubtedly the liver is frequently enlarged in this disease. Some of the enlargement is due to hyperæmia, and some to swelling of the hepatic cells. This latter is probably a true hypertrophy of the liver tissue."

Under DISEASES OF THE URINARY ORGANS, the subject which occupies most space is Bright's disease. There is much here that might be quoted, but the following remarks on the occurrence of Albumen in the urine may suffice :—

"It may perhaps be a necessary preliminary to inquire why it does not make its escape under ordinary circumstances, why the urine is not normally albuminous. We know that the other transudations from the blood contain albumen, and it seems at first sight strange that the urine does not. Some have even supposed that the urine as it is separated from the blood at the glomeruli is normally albuminous, and that in its subsequent passage through the tubules the albumen is absorbed by the epithelium just as the water is taken up and the urine concentrated. But this view is completely opposed to known facts. We have already seen that in chronic Bright's disease there is great destruction of the secreting epithelium, so that the glomeruli are, as it were, more near to the outlets of the tubules than normal; the urine secreted in these cases is very watery, and has, in fact, characters which are presumably those of the urine as it is separated at the glomeruli. But it is exactly in this class of cases that albumen is frequently absent from the urine. Again, in acute Bright's disease the renal epithelium is unduly active, and might be supposed to be peculiarly ready to absorb albumen as nourishment, but it is in these cases in which the quantity of urine is small that albumen is peculiarly apt to be present.

"It might be supposed that increased blood-pressure in the vessels of the glomeruli might lead to albuminuria, but this also does not seem to be the case. We shall afterwards see that in chronic nephritis the blood is at an excessive pressure, the excess of urine being partly due to this, but here albumen is frequently absent. Then in active hyperæmia there is increased pressure in the vessels of the glomeruli, but the urine although excessive in quantity contains no albumen.

"The occurrence of albumen seems, in fact, definitely related to the structural changes in the kidney. We have seen that the inflammation in its acute form affects the glomeruli primarily, and it is probably because the structure of the glomerulus is altered that the albumen escapes. In this view of it we infer that it is due to the structure of the glomerulus that the albumen does not get through in the normal kidney, and when this structure, especially the epithelium, is tampered with then albumen passes through as in ordinary transudations. It is to be presumed that other causes besides inflammation may interfere with the epithelium of the glomeruli. In acute febrile diseases it may partake in the general parenchymatous degeneration of the organs generally, and so allow of the passage of albumen. In passive hyperæmia also its nutrition will suffer and we may have albuminuria."

The question of increased arterial tension in Bright's disease is considered in some detail, and the various views concerning its causes are referred to. The conclusion come to is that the disease of the kidneys is the primary cause of the increased arterial tension, and that probably the condition of these organs alone is sufficient to account for it without the aid of any changes in the arteries throughout the body generally. Of these changes the existence of hypertrophy of the middle coat is recognized, but it is regarded rather as the effect than the cause of the increased tension.

The DISEASES OF THE FEMALE GENERATIVE ORGANS occupy about 42 pages, and those of the MALE GENERATIVE ORGANS about 10. In treating of puerperal fever it is stated that there is here a septic infection with or without pyæmic abscesses. To begin with, a local septic inflammation occurs in the uterus or other soft parts.

"From its local seat the process extends, and results in a more general infection. The path of extension and the resulting conditions are various. The septic inflammation may extend along the Fallopian tubes and septic pus finds its way into the peritoneum. The resulting peritonitis is rarely localized, but we have rather a general septic peritonitis with resulting absorption of the products, and septic poisoning. In other cases the organisms get into the uterine veins, and by the ordinary process of thrombosis, &c., we may have a pyæmia having the usual characters, with suppuration in the joints and miliary abscesses in the lungs and elsewhere. The thrombosis here nearly always starts from the veins of the placental part of the uterus. In some cases there appears to be a septic infection of the blood without any local abscesses from embolism. In other cases the inflammation extends to the connective tissue of the pelvis. We have a violent parametritis with acute suppuration. The inflammation extends by and bye to the peritoneum, and there is a violent septic peritonitis, just as when the extension is by the Fallopian tubes. The inflammation may extend through the diaphragm, and lead to a pleurisy.

"It will be seen that in all these cases there is a vigorous multiplication of the micrococci, and that there must consequently be a great formation of chemical products. An acute septic poisoning is the result, and it is probably this which leads to the more pronounced symptoms of puerperal fever."

The DISEASES OF THE BONES AND JOINTS come next in order. In discussing the absorption of bone, the following remarks occur—

"When, in a simple fracture, a considerable piece of bone is separated from all its attachments and all its blood-vessels torn across, then it will probably die. Necrosis does not occur in every case, as the observations of Macewen on TRANSPLANTATION OF BONE show that pieces of bone removed from one person to another may survive and become incorporated in their new position. But in order to their survival, the pieces must be small, so as to allow of ready penetration of blood-vessels into their medullary spaces. When pieces of considerable size are broken off they do not survive. Similarly in compound fractures pieces of bone are frequently separated from all their vascular connections and may be seen lying in the wound, with a dead white colour and a hard feeling when touched, being evidently dead. Or again, at the end of stumps necrosis of a piece of the bone may occur from osteomyelitis or otherwise. Or, as we shall see afterwards, periostitis may lead to extensive necrosis.

"Now, in all these cases, if acute suppurative inflammation be averted, and especially if septic inflammation be kept off, the dead bone may be absorbed. This is sometimes seen very plainly in cases of compound fracture where a piece of dead bone may become visibly eaten into and replaced by vascular granulations. It must occur frequently in simple fractures and at the ends of stumps. It is also not uncommon in necrosis from periostitis, but it will not occur unless the inflammation be moderate in intensity, just as new-formation occurs only in chronic inflammations."

The last section of the work is devoted to DISEASES OF THE SKIN AND ITS APPENDAGES. A large part of this is taken up with inflammations of the skin. These are divided into the Symptomatic inflammations, of which the small-pox eruption is taken as the type, the Inflammatory skin eruptions, the Traumatic and non-infective inflammations, and the Infective ones. The subject of syphilitic skin affections is treated of separately.

We have gone over the various sections of the work, chiefly with a view to giving our readers some knowledge of its contents, and we have quoted somewhat largely that they may be able to judge of the manner in which the subjects are handled.

It remains for us to refer to one or two special points in the general arrangement which have not hitherto been mentioned.

One of these is the form of the table of Contents. This gives really a brief summary of the whole book, so that the general plan on which each subject is treated may be readily seen. After reading any portion, the memory may be refreshed by turning to the contents. After each item in the contents the number of the page is inserted, so that the fuller statement of any subject sought for may at once be obtained.

Another feature is the free use of capitals in the text. Whenever in the midst of a paragraph or elsewhere a new subject is introduced, it is indicated by small capitals; these here take the place, to some extent of italics, which latter are very sparely used in the text.

Micro-Photography, including a description of the Wet Collodion and Gelatino-Bromide Process, together with the Best Methods of Mounting and Preparing Objects for Micro-Photography. By A. Cowley Malley, B.A., &c. London: H. K. Lewis. 1883.

In the first place, we must take exception to the title of "Micro-Photography" given to this little work, as by it the art of producing minute pictures of large objects is generally understood, whilst Mr. Cowley Malley's book treats, properly speaking, of *Photo-micrography*, or of the art of photographing the magnified image of microscopic objects, or of producing photo-micrographs. As the numerous descriptions of photo-micrography are at present scattered in innumerable scientific publications and periodicals, a small handbook containing a description of the most successful methods is very welcome. It is impossible to overrate the value of an art which enables its professor to fix almost instantaneously the enlarged image of microscopical objects, and to produce a large number of copies of it with the greatest ease, whilst drawings of the same objects might require the skill of an artist and days or weeks of time to execute them, in fact, might be impossible in the case of objects undergoing rapid changes. Moreover, the best drawing must always leave doubts as to its correctness.

Mr. Malley gives us first a chapter on the properties of lenses, which any one attempting photo-micrography is sure to have found already in his elementary handbook on the microscope, for assuredly no one need attempt photo-micrography unless he has made himself thoroughly familiar with the microscope, the optical appliances belonging to it and their uses, by a study of some good work on this subject, as well as by practical work. In the second chapter, Mr. Malley speaks of the microscope and illuminating apparatus which he recommends for photo-micrography—viz., a simple rigid stand without mechanical motions to the stage, although we do not see what objections could be urged against these movements, and how, without them, an object could be looked over systematically under high powers. The achromatic condensers recommended, if we except Powell and Lealand's immersion condenser, are also not those which microscopists would employ who are able to appreciate the modern refinements of illumination and the best workmanship. Mr. Malley goes even the length of saying that the various additions generally made to the first class stands are "an insult to the skilled microscopist." In this we cannot concur, although we are of opinion that they are in many cases quite superfluous. The author, in speaking of illumination, describes how a simple heliostat may be made out of a French clock for the purpose of photographing by sunlight; but he recommends the light of a paraffin lamp, as being always obtainable, requiring little care and expense, and as being sufficient for taking negatives of objects even with the twenty-fifth-inch objective, having a magnification of from 1,000 to 5,000 diameters, which will bear subsequent enlargements to 50,000 before the finest details become visible to the naked eye. This will be encouraging to those who think that either sun or electric light is indispensable for photo-micrography with high powers, as used to be the case before the introduction of "dry plates." Mr. Malley recommends a conical bellows camera, which is generally thought to be the best, and describes a wooden stand for the whole apparatus which he adopted finally after more than ten years' experience, and by means of which the operator is enabled to photograph with the camera either horizontal or vertical, the latter when the objects are in fluid and when the stage of the microscope has to be kept horizontal. According to our experience a better and more rigid arrangement for the latter case is a short stout body bent at a right angle and having a right angle prism, which arrangement allows of the stage of the microscope and the camera being both kept horizontal. Chapter IV treats of mounting and preparing objects, giving some of the best known methods, and calling attention to the staining of objects with suitable colours for photography. The wet collodion process is next referred to (Chap. V) as giving superior results over the dry plate process, although only suitable for high powers, when sunlight is available. The dry plate process is recommended on account of the short exposures required and its cleanliness and simplicity. Chapter VI informs the reader of the arrangement of the dark room, of the arrangement of the apparatus, and of the method employed by Mr. Malley for taking negatives of photo-micrographs. The author condemns here the employment of amplifying lenses, or of eyepieces to obtain increased magnification of the negative. This is hardly the experience of others; for instance, Mr. George E. Davies in his excellent handbook on *Practical Microscopy*, says that he finds the employment of such amplifiers makes no difference, and Dr. Woodward and others obtained with amplifiers results far superior to those obtained by Mr. Malley, if we are to judge of his success from the photo-micrographs forming the frontispiece of the little volume under review. To obtain with a high power objective and without the interpolation of an amplifying lens, a direct negative of sufficient size, necessitates the employment of a long camera, but a long posterior focus of the objective implies a shorter anterior focus or a shorter working distance, which is often not obtainable owing to the thickness of the covering glass. The fact is, that the result depends largely upon the quality of objective and amplifiers employed, and the proper correction of the former for the distance from the latter. Of course, there will be a fractional loss of light, but this may be compensated for by a longer exposure.

The seventh chapter discusses the defects in negatives, their causes and cures; and the last chapter, the production of positives according to various approved methods, as described in handbooks on photography and in the directions accompanying the dry plates. We would have liked to have had here more detailed information about the printing of good positives to be used as lantern transparencies, which are invaluable for lecture purposes. Mr. Malley's book, whilst containing little that is new, will be found useful chiefly by beginners in photo-micrography, and will thus serve its object. To succeed in photo-micrography requires, besides good microscopical apparatus and first rate object glasses, a thorough acquaintance with the microscope and with photography, and an immense amount of patience, which latter are only acquired in the course of years of serious work.

The type of the book is good, but there are several typographical errors in it, whilst one of the principal diagrams referred to in the text is awanting altogether.

The Diseases of the Prostate: their Pathology and Treatment. (Students' Edition). By SIR HENRY THOMPSON J. & A. Churchill. 1883.

ENCOURAGED by the success that attended the cheap issue of his Clinical Lectures, Sir Henry Thompson has now published this cheap edition of his famous prize essay on the Prostate Gland. By so doing, he has laid both students and practitioners under a further debt of gratitude. For, though this is not a student's book in the same sense as the other, it is so in perhaps a higher sense. If it is not a mere statement of the main facts of pathology and treatment, in a short and practical way adapted to the limited time and receptivity of the already overburdened student, it is still a book which, by its very elaboration and its academic form, cannot but prove most stimulating to the younger members of the profession, and will lead them to a more enthusiastic study of anatomical details, to a more accurate observation of pathological processes, and to a greater interest in, and reverence for, the works of the older surgeons.

For this edition, all the chapters have been carefully revised and added to, and a new chapter has been specially written, embodying the author's description of a new operation for the relief of the very distressing symptoms which accompany extreme cases of hypertrophy of the prostate. In some of these cases in which "the patient becomes the victim of a vicious circle of actions, in which an absolutely indispensable remedy, the catheter, aggravates the inflammation of the bladder, which therefore in its turn demands the instrument with increasing frequency, it occurred to me, that were it possible to suspend all action on the part of the bladder for a few days—to get rid of all accumulation of urine from the organ—to allay the constant and painful want to pass urine, and also, at the same time, to abolish catheterism altogether, with its irritating effect on the urethra, the inflammation of the bladder might subside, and its tolerance of urine might largely increase." With these objects in view, Sir Henry Thompson opens the urethra at the anterior part of the prostatic portion, by a vertical incision in the perinæum, using a grooved staff as for median lithotomy. The urethra being opened for about half an inch, the fore-finger is insinuated into the bladder and its interior explored. A large vulcanized india-rubber catheter, No. 18 or 20 (English scale), should then be introduced and retained for a few days. The relief given by this operation is very great, and the risks of it are slight.

The illustrations are numerous, and include one of the author's new aspirators for removing fragments of calculi from the bladder. In every respect we heartily commend the book, and wish it as much success as the former venture.

REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

WESTERN INFIRMARY.

REPORTS UNDER THE SUPERVISION OF J. LINDSAY STEVEN, M.B.

FROM PROFESSOR GAIRDNER'S WARDS.

A CASE OF CEREBRO-SPINAL MENINGITIS. [Reported by Mr. J. Lindsay Steven, M.B.]—This case is of interest for two reasons, first, because the disease is rare in this country; and second, because it presents several points of great similarity to one which Dr. Finlayson brought under the notice of the Glasgow Pathological and Clinical Society some little time ago. There was no opportunity for making anything like complete clinical observations, and the following note, inserted in the Infirmary Pathological Journal by Dr. Gairdner, contains all that was made out concerning the case during life.

"Peter D., æt. 2, admitted only to die, and with no information as to antecedents. Seen by Dr. Gairdner at the visit preceding death in an obviously extreme state of exhaustion, pallid to a high degree, semi-comatose or completely unable to speak, with dilated but equal pupils nearly, if not absolutely, insensible to light; *tache cérébrale* not producible to any appreciable degree; fontanelle scarcely closed, otherwise not remarkable; no convulsion nor any cries observed; no paralysis; no squint. The only account that could be obtained was that the child had been wasting before admission; but how long the illness had lasted was unascertained."

The following is Mr. Steven's account of the *post-mortem* examination, which was made on the 3rd of May.

External appearances.—There is great emaciation; and on the skin of both forearms there are several small bluish or "blae-coloured" spots of varying size, the largest being about the size of a split-pea.

Chest.—A small quantity of fluid is found in the pericardium, and the heart, weighing two ounces, presents normal characters.

The left lung is generally slightly adherent, but, for a space corresponding to one and a half inch downwards from the upper margin of its lower lobe, and extending right round from the posterior to the anterior margin, it is very firmly so. On separating this the lung tissue is torn to some extent, and both the surface of the lung and that of the chest wall are covered by projecting fibrous bands, which have a tendinous, perhaps atheromatous, look. The lung tissue beneath this area is slightly solidified and fleshy. The whole appearance is suggestive of an old empyema.

The right lung is slightly adherent throughout, but otherwise normal.

Abdomen.—The spleen is normal in appearance and weighs ¾ of an ounce. The kidneys are healthy, and weigh respectively 1¾ ounce. The liver is very pale, its lobules being distinctly mapped out by their pale circumferences, and weighs 15¾ ounces. The intestines present quite healthy appearances throughout.

Head.—On cutting into the dura mater about 7 or 8 ounces in all of a reddish serous fluid escapes, and, on reflecting it, the internal surface is seen to be covered (more especially on the left side) by an abundant, soft, red exudation, which, in some situations, gives to it the appearance of the granulating surface of an ulcer. In other situations, distinct bright red hæmorrhagic spots are noted, covered by a thin translucent pellicle. Where the colour is not bright red, the exudation resembles very closely soft recent fibrine; and in all situations it is easily peeled off from the firm tissue of the dura mater beneath. The whole condition bears a very striking resemblance to pachymeningitis hæmorrhagica.

The vessels on the surface of the brain are intensely injected. Beneath the arachnoid there is an abundant grey, slightly yellowish exudation, matting together, and often obscuring the outline of, the convolutions. This exudation is everywhere present on the vault of the cerebrum, and is well seen in the sulci of the longitudinal fissure. It is also well seen on the upper surface of the cerebellum. At the base of the brain the same condition of matters is noted, and here, too, it is seen that the exudation may frequently be followed as a white streak along the course of the injected vessels of the surface, often obscuring their outline to some extent. The structures at the base are considerably obscured, and the fissures of

Sylvius are firmly glued together. Around the optic chiasma the appearance ot the exudation approaches somewhat that of pus.

The brain tissue generally is very soft. On opening into the left lateral ventricle it is seen to contain a small quantity of turbid fluid, and the choroid plexus has an opaque appearance. Similar appearances are noted on the right side. On cutting into the brain substance, the injected vessels of the pia mater are well seen dipping down into the sulci between the convolutions; and in many of the sulci the surface of the grey matter presents a softened, inflamed appearance. In some of the convolutions, too, numerous minute red points are observed in the grey matter, suggestive of the occurrence of small hæmorrhages.

In the spinal cord, too, similar conditions are observed—the exudation in this case being much more abundant on the posterior, than on the anterior, aspect of the cord. The tissue of the cord is exceedingly soft; the dura mater presents healthy characters.

Some of the exudation from the cord is immediately taken and examined by Weigert's method for organisms. On completing the process, it is found that numerous very small well defined micrococci are present among the pus corpuscles.

The fluid from the arachnoid cavity is found to consist of an almost equal admixture of red blood corpuscles, and highly granular (fatty) pus cells.

FROM DR. FINLAYSON'S WARDS.

A CASE OF ADDISON'S DISEASE. [Reported by Mr. Alfred Williams, *pro tem.* House Physician.]—Allan L., aged 38, a blacksmith, admitted 14th March, 1883, presenting a marked discoloration of his skin; but except this, he has nothing to complain of, and says that, had it not been for the advice he has received, he would now be back at his work.

He attributes his present condition to a strain he got while at his work in January, 1882. He was carrying a very heavy iron frame with another man, and after putting it down, he felt a stitch in his right side, and remarked so to his fellow-workman. He went on with his work, but the pain became worse, and on the third day after he sustained the injury, he had to stop working. The pain was felt in the right hypochondriac region, and was especially bad when he stooped to pick up his tools; it was also severe in the night time if he lay on his left side, but by lying always on the right side he got relief. He consulted a medical man who, after examining him, told him his liver was a little swollen, and painted the part with iodine. Patient remained away from his work for eight days, and by that time the pain had left him, except if he lay on his left side at night. Neither the patient nor his wife can state definitely when the discoloration of the skin was first noticed. It seems to have come on gradually, but soon after this illness; and at first his friends accused him of not having washed his face thoroughly. He continued at his employment, and was quite equal to it until June, 1882. The pain then gradually came on again, and increased in severity, until it compelled him to stop work. He again consulted his doctor who, so far as patient knows, was unable to detect any swelling. A galvanic battery was applied, holding one sponge over the spine, and moving the other over the right hypochondriac region. This caused great pain at one particular spot, situated just below the lower border of the cartilages of the last ribs, and in about the nipple line. The spot could be covered by the sponge. Iodine was applied, and patient returned to his work after eight days' absence quite free of the pain, except when lying on his left side. It had troubled him when in this position from the beginning, and continued to do so till a few weeks ago, when the pain left him altogether.

About the middle of July his skin became very dark in colour.

Early in August he felt a hard lump in his left cheek: after poulticing this became softer, and in November a quantity of pus was discharged below the lower jaw; a sinus still remains in this situation, from which pus occasionally comes. No pieces of dead bone have ever come from it.

About the end of August or the beginning of September he suffered from a sharp attack of diarrhœa, which confined him to the house for three days. After this he worked on till near the end of December, when he had an attack, which seems to have resembled typhoid fever in some respects. He went back to his work on 8th January, and got a lighter job to do. He still felt rather weak, but worked on for three weeks, and then, on coming home one Saturday, he was seized with a headache and severe shivering, which confined him to bed for a day or two. Returning to his work on the Tuesday, he struggled on with it till the end of the week, since which he has never done any.

For three or four days he had pains on the right side of his back, extending down the right groin and into the right testicle; these pains were relieved by turpentine stupes. He lay in his bed for three weeks, being almost too weak to raise his head off the pillow. When lying on his back, he felt quite well and fit to do his work, but when he raised his head up he at once felt feeble, and everything seemed to swim before his eyes; he also felt sick, and occasionally vomited. About this time he first noticed discoloration in the skin of his abdomen, and that his privates were much darker than they previously had been.

After being in bed for three weeks he got up about the end of February. He then noticed his arms and legs were very much wasted. From then up to the present time he has been gaining flesh and getting stronger.

Previous to this illness, patient has always been healthy. He used occasionally to have attacks of sickness and vomiting, but it was always after some excess in drinking. Some years ago he was troubled with stitch in his left side, which left him after one application of dry cupping. When young he had scarlet fever. He denies having had any kind of venereal disease, but 20 years ago he had a suppurating bubo in his right groin, where a cicatrix still remains. He has been accustomed to take whisky pretty freely. He has never been abroad. His father is still alive, and has always been healthy, though subject to attacks of "lumbago." His mother died in childbed when patient was young. He only had one sister, who died at the age of twenty-two of "inflammation of the bowels."

At present patient does not appear emaciated, there seems to be the usual amount of subcutaneous fat found in a working man. His skin is cool and soft, and almost over the whole of his body is tinged more or less deeply with a brownish pigment. In the face the pigment is very abundant, giving patient the complexion of an Arab. The discoloration is especially distinct on the alæ of the nose, and where these join the cheeks, also in the skin below each eye; beneath the right eye there is a well defined patch of brown pigmentation. The conjunctivæ appear very white, and present a marked contrast to the surrounding skin. The irides have an outer bluish-green zone, and an inner zone surrounding the pupil of a gamboge colour.

The brown colour of his face shades off, and becomes lighter in those parts that are covered, such as the upper part of his forehead covered by the hair, and the skin behind the ears.

The skin of his neck is very dark behind, but gradually gets of a lighter shade as we pass forwards. At the back there are two small cicatrices caused by burns, these are discoloured slightly, but around them there is a distinct zone of skin where the pigment is more abundant. The discoloration is of a mottled character when looked at closely, and there are a few spots where the pigment is quite black. The discoloration of the neck shades rather abruptly into that of the back in the upper part of which it is only slightly marked. There is an ill defined patch of brownish discoloration about the lower angle of each scapula. A streak of pigmentation runs down the lower part of the back in the middle line, the colour is deeper over each spinous process.

The spinous processes of the last dorsal and the first two lumbar vertebræ are bifid and prominent, and over these the pigmentation is well marked. In the lower part of the lumbar region on the right side there is an area of skin where the pigment is very abundant, it corresponds to the place where a turpentine stupe was applied about seven weeks ago. Between the two gluteal eminences the skin is almost black. Over each tuber ischii there is a patch of very deeply discoloured skin. Above each clavicle, but more so above the left than the right, the skin is discoloured where the braces cross, and above the point of each shoulder, especially the right, the pigment is more abundant. The skin on the front of the chest is very slightly tinged, the colour becomes more marked as we approach the shoulders, and is very dark in the axillary fold. The nipples are of brownish-black colour, and there is a slight secondary areola. On the right side of the chest there are one or two liver coloured patches.

As we pass from the chest to the abdomen the colour becomes gradually darker. There is a narrow streak of pigment extending from the xiphoid cartilage to the umbilicus, and from there to the pelvis, more distinct below the umbilicus. The navel itself is also darker than usual.

The skin in the right and left iliac and hypogastric regions is much darker than that in the upper part of the abdomen, from which it is separated by a well defined margin. Seven weeks ago patient had hot water cloths applied to this part. To the right, and a little below the umbilicus, there is a patch about an inch long where the skin is almost black. In the right groin there is a cicatrix in which pigment is deposited, but not to a greater extent than in the surrounding skin.

The scrotum is more deeply pigmented than any other portion of skin. On the anterior surface the colour is blackish-brown, and becomes lighter if the skin is stretched. The skin of the penis is also of a dark brown colour, but not so dark as the scrotum. Over the great trochanter on the right side there is a round patch of dark skin; this is the side on which patient used to sleep.

The skin in the groins and about the anterior spines of the ilia is of a light brown colour. It gets lighter still as we pass down the leg, and is hardly noticeable about the middle of the thighs, but darkens again around the knee joint. The backs of the legs are more pigmented than the surrounding skin. On the outer side of the left leg, at the place where patient had a burn about three months ago, there are two patches of almost black skin. About each malleolus the skin is slightly pigmented. In the feet the discoloration is most marked along the ridge of the dorsum, and in the toes on the upper aspect of each joint there is a small patch of discoloured skin.

In the upper extremities the discoloration is more marked in the right than in the left arm; it is pretty distinct in the skin covering the deltoid, but more so on the inner side of the arm extending down from the axilla. On the outer side of the right arm there are a few streaks of a deep brown colour. The skin over and around the olecranon is deeply pigmented. In one arm there is a slight cicatrix in this situation resulting from a burn; this is free from pigmentary deposit, but there is a well-defined areola of dark skin around it. The forearm, especially on the right side, is more deeply pigmented than the upper arm, the colour being deeper on the extensor than on the flexor surface of the limb. There are numerous small patches of a dark brown colour marking places where patient has been slightly burned. There are also one or two cicatrices resulting from cuts, but these are free from the pigment, nor is the immediately surrounding skin more deeply pigmented. The backs of his hands are very deeply pigmented, and look as though they were stained by the juice of walnut hulls. The colour is especially dark over the knuckles. The palmar surface of the hand is almost free from pigment; on the sides of some of the fingers there is an abrupt boundary line between the pigmented skin of the dorsum and the normal skin of the palm.

The colour of his skin varies at different times in its depth. It is always darker when the surface is cold—this was especially noticeable when the bed clothes were drawn down to expose the abdomen.

There is apparently no disease in the osseous system, unless the sinus below the jaw has some connection with dead bone. He feels quite strong, and is able to walk about without feeling tired.

His temperature since admission has been rather below the normal, generally 98° or 98·2°, and only on one occasion was it 98·6°.

His blood was examined microscopically, but no relative alteration in the number of the red and white corpuscles was noticed. With the hæmochromomètre, the colour of the standard solution was reached by diluting the 20 c.m. of blood up to 60.

His appetite is good, and he takes his food well. His tongue is not furred, but appears as if it were slightly stained with ink. The mucous membrane inside the lips is of a mottled brown colour, and on the inner side of each cheek opposite the teeth the pigmentation is of a blackish tinge. The pharyngeal mucous membrane is congested, but not pigmented. There is no vomiting or pyrosis. Once or twice he has been troubled with hiccup, but it has never been a prominent symptom. His bowels are regular.

A careful physical examination was made of the organs of the chest and abdomen, but without detecting anything worthy of record.

MEETINGS OF SOCIETIES.

GLASGOW SOUTHERN MEDICAL SOCIETY.

22ND FEBRUARY, 1883.

MEETING VIII.—SESSION 1882-83.

DR. BARRAS, *President, in the Chair.*

DR. A. NAPIER read a paper on VACCINAL ERUPTIONS, which will be found on page 424.

Dr. E. Macmillan had used calf lymph solely for several years, and had not noted any specific rash following its employment. He had had one case of erysipelas succeeding vaccination, but due to exposure to cold.

Dr. A. T. Smith used calf lymph and lymph a few removes therefrom, and stated his impression that rashes of a mild papular character, appearing about the eighth or tenth day, were very frequent.

Dr. Glaister had very seldom seen any eruption following any of his vaccinations, and always felt very anxious when he did see it. He used chiefly humanized lymph, and thought that, even with the greatest care and watchfulness, it was difficult to estimate accurately the elements that enter into our lymph or the probable effects they will produce. Children, about the period when they are usually brought for vaccination, are peculiarly prone to exhibit rashes of various kinds, and vaccination may simply determine the appearance of such rash.

Dr. M'Vail considered the use of animal lymph a mistake, and held that Dr. Napier's statistics showed this. The storage of lymph in tubes, on points, or in any other way, he regarded as an essentially bad procedure. No matter how stored, lymph at once begins to decompose when removed from the vesicle; and the eruptions which had been referred to he thought were due simply to the introduction of the products of decomposition into the system. In view of modern antiseptic teaching, the surgeon would be considered culpable who approached a wound with a knife or other instrument marked with even a small dry crust of blood or discharge, which of course must be the seat of some degree of decomposition. Why should we act differently in regard to vaccination? Vaccination can be performed safely only from arm to arm. In his experience of five or six thousand arm to arm vaccinations at the Western Infirmary, only six had come back showing eruptions—one of these was a case of erysipelas, the remainder, eczema. He was careful never to vaccinate if the child showed the slightest trace of eczema (behind the ears, in the groins, &c.), as vaccination is apt to aggravate the skin eruption, which in turn interferes with the proper development of the vesicle.

Dr. Pearson said he had been using the same continuity of lymph for the past twenty years, and kept it stored in capillary tubes. He had not had a case in which he had failed to vaccinate at the first attempt, for at least sixteen years; and he had never seen rashes following the operation.

The President said he had used lymph stored by all the ordinary methods (tubes, points, glass squares), and had much the same measure of success with all of them. He thought the appearance of rashes due most frequently to circumstances other than vaccination. He did not hesitate to vaccinate in presence of eczema, and even found vaccination somewhat remedial in such cases.

Dr. Napier, in reply, said he had hoped that the discussion would have taken a somewhat different turn from that which it did take. He should have liked to have heard debated the question whether the eruptions he had described, presenting as they did, tolerably constant features, had any specific relation to the vaccine virus. He himself was of the opinion that they had such a relation. Dr. M'Vail's views as to the storage of lymph were certainly well worthy of consideration; but he thought too much was made of them. No one of course would dream of deliberately introducing decomposing material into the system; but there was but little risk of this being done in using lymph in the ordinary way. In his case of erythema exudativum, just described, the lymph had been used certainly within three hours after being taken, and in that time very little decomposition could have taken place. Further, eruptions following vaccination with animal lymph could scarcely be considered as due to the inoculation of decomposing materials, as it was well known that animal lymph (in tubes), the kind whose use is most frequently succeeded by rashes, must be used as fresh as possible, that in fact it will almost certainly fail if kept longer than two or three days; while humanized lymph, which but rarely causes the appearance of any specific rash, may be used with perfect safety, and with no loss of its infecting power, weeks or even months after collection. As regards infantile eczema, he had occasion to see it very frequently at Anderson's College Dispensary, and found that whenever it occurred *after* vaccination, it was ascribed to that operation. He never lost an opportunity of proving to parents the error of this view. He believed that in most cases such an eruption is due rather to dirt and early weaning, improper feeding, and the constitutional excitability connected with dentition; but that occasionally vaccination determines the appearance of an eczema in a subject so predisposed.

MEDICAL ITEMS.

UNDER THE DIRECTION OF

ALEX. NAPIER, M.D.

The Pathology of Erysipelas: Inoculation Experiments upon the Human Subject.—Fehleisen (*Die Ätiologie des Erysipels.* Berlin: Th. Fischer, 1883) finds in erysipelas the lymphatic vessels of the skin and subcutaneous cellular tissue as well as the superficial layer of the corium filled with micrococci in chains, and when these occur in large number they also invade the lymphatic spaces and interstices of the tissue, but do not at any time enter the blood-vessels. Most of the vesicles occurring in the disease contain only pure serum, but others contain the specific micrococci as well as other forms.

The micrococci of erysipelas can be separated by cultivation from the chain-forming species found in acute suppurations, pyæmia, and phlegmonous inflammation. The fact that these organisms retain their vitality, and even multiply outside of animal bodies, as on vegetables, explains the occurrence of epidemic erysipelas in hospitals, &c.

Fehleisen inoculated nine rabbits with cultivated infusions, with the result in all cases but one of setting up in from 36 to 48 hours a regular erysipelas of from 6 to 10 days' duration. None of the cases proved fatal. In one of the rabbits the whole ear was amputated when the erysipelas had extended half way down the ear from the point of inoculation at the tip. This animal had regained perfect health in 12 hours.

Seven patients, the subjects of irremovable malignant tumours, were inoculated with the cultivated infusion, and in only one patient did the inoculation fail, though twice tried. This patient was already subject to recurring attacks of erysipelas of the face, and had passed through an attack between two and three months before. Of the successful cases two were more than once inoculated. In one, the first inoculation, on the 7th October, took, but subsequent attempts on the 24th October and 7th November failed; the other had had erysipelas in December, 1881, but inoculation on the 8th October took, though a second, 33 days later, failed.

From these experiments it would appear that an attack of erysipelas confers an immunity from the disease for a short time only. The period of incubation observed, from the time of inoculation to appearance of redness and rigor, varied between 15 and 61 hours.

It was found that 20 seconds' contact with a 3 per cent solution of carbolic acid had no effect upon the virulence of the micrococci, but that 30 seconds' contact "retarded and disturbed" their action, while 45 seconds' contact destroyed it, as did also from 10 to 15 seconds' contact with a 1-1000 solution of corrosive sublimate. The use of these antiseptics, therefore, should guarantee safety from infection.

With regard to the effect of the erysipelas upon the morbid growths (sarcoma, cancer, and lupus), it was found that in all cases the progress of the disease was considerably retarded, subcutaneous and cutaneous nodules and infected glands being especially diminished in size. In two of the cases, however, —recurrent cancer of the breast—the erysipelas was complicated with pleurisy. Former communications on this point are—Fehleisen in *Deut. Zeitschr. f. Chir.*, 1882, Bd. xvi, hft. 5 and 6; and in *Sitzungb. Würzburger Phys. Med. Gesellsch.*, 1882; *Centralbl. f. Chir.*, 1882, No. 48. Sarubin, *Centralbl. f. Chir.*, 1881, No. 48.—*Centralbl. f. Chir.*, 1883, No. 9.—D.M'P.

Germs of Enteric Fever.—Dr. James C. Wilson, in the *American Journal of the Medical Sciences* for April 1883, shows that although the nature of the germ that gives rise to enteric fever is unknown, many facts in its natural history are established by abundant proof. Of these, the following have a direct bearing upon this subject:—

1. It is invariably derived from a previous case of enteric fever.
2. It is eliminated with the fæcal discharges.
3. It is not capable of producing enteric fever at once in susceptible persons exposed to it, but must undergo changes outside the body before it acquires this power.
4. It retains its activity in favourable situations for a lengthened period, the requirements to this end being decomposing animal matter, especially fæcal discharges and moisture.
5. In such situations it is capable of reproducing itself.

These are the facts which indicate with singular directness the true measures necessary to prevent the spread of the disease, the efficient prophylaxis—namely, the disinfection of the stools.

Gonococcus, the Germ of Gonorrhœa. Inoculation Experiments on the Human Subject, with Post-Mortem Examination.—In a communication laid before the Würzburg Phys.-Med. Society, 23rd Sept., 1882, Bockhart describes the *post-mortem* appearances of acute gonorrhœa induced by injection of pure cultivated infusion of the "gonococci," previously described by Neisser, Krause, and Leistikow.

With a cultivated infusion of the fourth generation he injected the previously healthy urethra of a patient 46 years of age, suffering from an advanced stage of general paralysis, with the result of inducing a regular acute gonorrhœa, with abundant secretion of yellow pus. This lasted till the death of the patient, which took place ten days later, from inter-current hypostatic pneumonia. Daily examination of the secretion during that time showed the constant presence of the specific micrococcus.

At the *post-mortem* examination the urethra was found filled for 2¼ inches from the orifice with a viscid, sanguineo-purulent exudation. The corpus spongiosum was swollen and hyperæmic, but the corpora cavernosa were free from all appearances of inflammation.

Microscopic examination under moderate power showed the mucous membrane, especially at the floor of the fossa navicularis, swollen and cloudy, and the mucous and submucous tissue, as well as the connective tissue, septa, and the blood sinuses of the corpus spongiosum full of wander-cells. Stained sections showed the lymphatics, and some of the blood sinuses, and many of the wander-cells very deeply stained, and higher powers (Siebert's 1-12th homogeneous immersion) showed the lymphatics and sinuses to be thrombosed, and these cells to be filled with gonococci. With the higher power it was also found that the nuclei of most of the other leucocytes contained from 4 to 12 gonococci, mostly in rolls, seldom isolated. In the exudation there were found, mixed with pus corpuscles, free from micro-organisms, numerous agglomerative masses of gonococci, which were regarded as nuclei of ruptured leucocytes. The gonococci were now seen within the epithelial cells.

Bockhart's summary of this subject is as follows:—

1. Gonococci are the pathogenetic bacteria of gonorrhœal affections.

2. Coming in contact with the mucous membrane, they enter (probably between the epithelial cells) the lymph channels of the mucous membrane and submucous tissue of the fossa navicularis, there to multiply and induce inflammation with exudation of the colourless blood corpuscles.

3. They enter these corpuscles and their nuclei, and so gain entrance into the blood-vessels and connective tissue of the mucous membrane and corpus spongiosum, towards the bladder.

4. They finally destroy these white corpuscles, either in the tissues, in their progress through the epithelium, or in the purulent excretion. They are themselves destroyed, as well as those which do not enter the leucocytes, probably in the tissue or in the blood-vessels.

Neisser and Arning maintain that the gonococci are found within the epithelial cells, which Bockhart, as noted above, distinctly denies.—(*Centralbl. f. Chir.*, 1883, No. 9.)—D. M'P.

Hahn's Operation for Excision of the Knee.—Dr. E. Hahn holds that in certain cases of partial resection on account of recent injuries, and especially gunshot wounds, in which it is often practicable to retain the function of the joint, Langenbeck's internal longitudinal incision is the best, as it leaves the extensor apparatus nearly intact. In other cases, in which operation is demanded on account of purulent inflammation of the joint with caries, or of fungous inflammation, a movable joint is not looked for; the diseased parts have to be removed as thoroughly as possible, after which a firm bony ankylosis is the best result. Here the best method of operating will be that which permits most easily and thoroughly the removal of the diseased synovial membrane. Volkmann accomplishes this by dividing the patella cross-wise, and thus exposing the interior of the joint. Hahn prefers the following method:—The limb to be operated on, emptied of blood by Esmarch's procedure, is placed in the fully extended position; with an ordinary amputating knife a transverse incision is made immediately above the patella, dividing the quadriceps tendon, starting from the posterior part of the inner side of the joint (the posterior end of the inner joint line) and ending at the posterior part of its outer side. This incision must at all points extend clear down to the bone. The knee is now strongly flexed, when it is found that the whole of the interior of the joint is widely exposed. The diseased parts are then easily removed; with the first incision the upper part of the synovial pouch is already in part removed. This incision has the advantage over Volkmann's of being more easily made, as the patella has not to be sawn through, nor has it afterwards, as in Volkmann's operation, to be bored and drawn together with sutures. The functions of the quadriceps are restored after this operation quite as fully as after division of the patella. Over the ordinary H incision, and the operation by an anterior flap, Hahn's operation has these advantages:—

1. The occurrence of union by first intention is more probable, as the incision is not situated over the ends of the affected bones; and, further, the soft parts above the patella seem better suited for such primary union.

2. The deepest part of the wound can be drained directly in the joint-line—a very important matter. To expose the upper synovial pouch also, by the anterior flap operation, the ends of the incision have to be prolonged so far upwards that they come considerably above the line of the joint.

3. The patella is not drawn upwards, but remains on the joint-line, and adds to the firmness of the union.

4. Fewer arterial vessels are cut; it is always practicable, without fear of secondary hæmorrhage, to remove Esmarch's tube after applying the "Listerian" dressing, and without tying any vessels.

Hahn also draws attention to his method of fixing the ends of the bones together—by nailing. Since February, 1881, he has practised this method in every case, and has seen no disadvantage arising from it; on the contrary, he believes that the firm and steady apposition of the bones, obtained in this way, gives a quicker and more certain bony ankylosis. He uses nails of the best steel, about 3 mm. in thickness, and 10 cm. long, pointed like an ordinary trocar, and with a small head. These nails are carefully disinfected, grasped like a trocar and pushed through the skin over the head of the tibia; with a few strokes of a hammer they are then driven through tibia and femur. Usually three such nails are inserted parallel to each other. At the end of 14 days these nails are generally still firm. The first dressing is changed in 24 hours; the second usually remains on till the end of the third week. The nails can then readily be removed with forceps, by slightly rotating while extracting. By this time firm union has as a rule taken place.

In 23 cases Hahn has had three deaths: one man of 30 died four months after the operation from general tuberculosis; a child of five died 3 weeks after union by first intention had taken place, from diphtheria of the fauces and bronchitis crouposa; a child died fourteen days after the operation, from septicæmia, caused apparently by absorption from a "bone-drain." Of the remaining cases sixteen healed by first intention; in fourteen cases there was bony union. In four cases primary union did not take place, and two of those were among the three on which Hahn had operated by anterior flap. In two instances there was firm fibrous union; in two slight movement in the joint; in other two cases the result is not yet known. Hahn's patients' ages varied from 5 to 55. In children the fungous masses were usually removed, and the cartilages slightly "freshened" without disturbing their epiphysial attachments. As regards the diseases for which the operation was performed, in eighteen cases it was fungous inflammation of the joint, usually with caries; in three instances it was purulent inflammation of this joint with caries after gonorrhœa, acute articular rheumatism, and puerperal fever; and twice, angular bony ankylosis.—*Cbl. f. Chir.*, No. 29, 1882. Beilage.

The Virus of Symptomatic Charbon (Anthrax).—M. Franck read to the Société de Biologie a communication from M. Arloing upon the changes undergone by the virus of symptomatic charbon under the influences of certain destructive agents.

The numerous experiments which M. Arloing has performed have led him to the following conclusions:—The virus resists the influence of cold, while heat has a manifest influence over it, and may be counted upon to transform the virus into an attenuated or vaccine virus. M. A. has also studied the influence of antiseptic agents upon the poison. Among these agents there are some which have not had the results expected of them, and which had no effect upon it. Such are, *e.g.*, alcohol, camphor, carbolic acid, oxygenated water, &c., which, according to M. A., do not destroy the virulence of the bacteria.

M. Paul Bert declared that his own experiments had taught him the contrary in reference to oxygenated water.—*Gaz. des Hôp.* 20th Feb., 1883, p. 164.—J. L. S.

A Simple Means of Checking Pulmonary Hæmorrhage with Shawl Straps.—Dr. H. Holbrook Curtis gives, in the *New York Medical Record*, a novel way of arresting pulmonary hæmorrhage. Called in a case of emergency, Dr. Curtis purchased a pair of ordinary shawl straps punched with holes a quarter of an inch apart, and braided three strands of drainage-tubing, making two cords of as many feet long. He laid a folded napkin over each femoral vein just below the fold of the groin, and adjusted the straps above the thighs as high up as possible, so that the buckles would be over the napkins. The straps were tightened enough to stop the venous return, without interfering with the arterial supply of the extremities. Then the arms near the shoulders were bound by the rubber tubing. The hæmorrhage was checked almost immediately, and in about five minutes the straps and tubing were loosened. This was no sooner accomplished than the patient complained of a great shock to "the sore place," and the bleeding recommenced. The same procedure checked it as before. In about five minutes, the extremities becoming markedly cyanotic, the straps were loosened, a hole at a time, when no hæmorrhage recurred. The shallow and difficult respiration was greatly relieved by keeping an arm and the opposite leg strapped. As soon as a member became cyanotic, the strap was changed to the opposite side.—J. L. S.

Treatment of Puerperal Mastitis by Iodide of Lead Ointment.—In the *American Journal of Obstetrics*, Dr. T. T. Gaunt expresses his disappointment at the ill success of belladonna in checking the secretion of milk, but reports good effects from the use of iodide of lead. He says—"The breast being thoroughly dried and perfectly cleansed, we smear its surface with the officinal ointment of the iodide of lead, and then rub it in until a considerable quantity is absorbed. Soak a piece of lint, of a size sufficient to cover the breast, in the following solution—acetate of lead, from ʒij to ℥ss to the pint of a 1-4 solution of alcohol or eau-de-Cologne. If there be much pain, it is often well to apply an ice bag over the lint covering the breast. The lint should be frequently dipped in the lead lotion. . . . It is common for the patient, who has been exhausted by pain and loss of sleep, to fall into a refreshing slumber soon after the application. In the course of three or four hours the breast may be completely emptied by an experienced hand, the ointment being used as a lubricant during the manipulation. By applying the iodide freely twice or thrice daily, the secretion will be gone in less than one week, as a rule. . . . A point of considerable moment is the partial anæsthesia it is capable of inducing, which enables us to empty the glands, where before some slight pressure was badly borne.—*Boston Med. and Surg. Journ.* 18th January, 1883.—D. M'P.

The Use of Iodine as a Stomachic Sedative.—The employment of iodine for the relief of the vomiting of pregnancy has been somewhat in vogue for a number of years. And while the success attending its use has been pointed out with more or less enthusiasm its exact value has never been established.

Dr. T. T. Gaunt (*American Journal of the Medical Sciences* for April, 1883), has for a number of years been employing the tincture of iodine in drop doses in nearly all forms of emesis, and reports thirteen cases of the most varied character in all of which vomiting was promptly arrested by the use of this drug.

The Contagiousness of Phthisis.—Dr. Lendet, of Rouen, at the International Congress of Hygiene, at Geneva, gave an account of fifty-six families all belonging to the better classes, of which, in fifteen, the husband was tubercular at marriage and the wife then healthy, and in forty-one the wife was alone affected. Of the first fifteen, in five the wife became affected, two having had relatives dying of consumption, and a third not becoming affected till ten years after her husband's death. Of the other series only three husbands became affected, and of these one had lost a sister of tuberculosis. Of five wives who became tuberculous after marriage, four had children; but only one lost any from tuberculosis. Of the ten wives who escaped, nine had children, and five lost one or more from this disease. He concludes as follows:—(1.) Wives contract tuberculosis more readily from their husbands than husbands from their wives. (2.) Wives who are not themselves affected may give birth to children who die of phthisis. (3.) Marriage hastens the fatal termination of the disease. (4.) Tuberculosis may often develop among different members of the same family, at short intervals, without hereditary predisposition.—*Birmingham Med. Rev.* March, 1883.—J. A. A.

The Action of Sodium Salicylate on the Heart.—Prof. Maragliano, of Genoa, has investigated this subject afresh, the results of previous observers being in many points contradictory. Some make out that the heart is weakened, others that it is strengthened by the salicylate. Liebermeister, for instance, in *Zeimssen's Cyclopædia* (vol. i), says he never gives the salt to a patient whose heart is weak, and he advises that it should never be prescribed in such cases. Maragliano made a triple series of observations to determine the question. In the first, patients who were taking the salicylate regularly were examined sphygmographically before, during, and after the exhibition of the dose, morning and evening. In the second, the pulse-curve of other persons was taken, before and after the exhibition of a single five-gramme dose. In the third, the arterial pressure was measured by Basch's method before and after the exhibition of a single five-gramme dose. The results showed that—(1) As the dose was gradually increased in patients of the first class, the pulse became progressively stronger, and the systolic line of the sphygmographic trace became higher. (2) After the single dose the pulse was stronger and the systolic line higher; the increase appeared an hour after the dose, reached its maximum in two to three hours, and disappeared after three to five hours; the normal dicrotism was accentuated, and often passed into tricrotism. (3) The arterial pressure rose about an hour after the exhibition of a single dose, and returned to the normal about three hours afterwards. The rise in pressure varied between ten and twenty millimetres of mercury. These results indicate unmistakably that no depressing influence is exerted on the heart by the salicylate. (*Centralb. f. d. Med. Wiss.*, 2nd Dec., 1882.) *The Practitioner.* January, 1883.

Anæsthetic Mixtures for Small Operations.—"It is often desirable to apply locally some anæsthetic material to deaden the sensibility sufficiently for small operations. There are various expedients proposed for this purpose. We do not now refer to the use of ether spray, but to various liquids which may be applied, and the sense of pain so far obtunded as to permit incisions without experiencing any other sensation than the mere touch. The mixture of chloral and camphor is often useful. When equal parts of chloral and camphor are triturated together, a clear, somewhat viscid, transparent solution results. This solution has considerable solvent power, and will take up a comparatively large proportion of morphia. Chloroform may also be added to it without precipitation of any portion of the dissolved constituents. Thus:—℞. Chloral., Camphor., āā ʒij; Morphiæ sulph., ʒss; Chloroformi, ℥j.—M. This may be applied with a camel's hair brush over the area to be incised, allowed to dry, and reapplied as freely as may be necessary to render the part insensible to pain.

"Amongst the anæsthetic mixtures for surgical purposes proposed by Prof. Redier, are solutions of camphor in ether and in chloroform. According to Redier, one drachm of camphor may be dissolved in two drachms of ether, or the same quantity of camphor in two drachms of chloroform. A useful anæsthetic mixture is prepared by the addition of crystallized acetic acid to chloroform, in the proportion of one part of the acid to twenty parts of chloroform. These anæsthetic solutions are applied by the brush freely over the part the seat of pain, or to be incised. In some instances it may be better to moisten a cloth or some cotton and allow it to remain for some time in contact with the part."—*Med. News.* 10th February, 1883.

The Use of Oxygenated Water.—MM. Paul Bert and Regnard have recorded in a pamphlet communicated to the Société de Biologie, all the surgical cases in which oxygenated water has been employed.

M. Bert asks, in connection with this, if oxygenated water could not likewise be used internally as a medicine, in draught, for example, in cases of phthisis, and as an enema in certain chronic diarrhœas, that of Cochin China in particular, which, as is well known, depends upon the presence of special micro-organisms in the lower part of the large intestine.—*Gazette des Hôpitaux.* 6th March, 1883, p. 213.—J. L. S.

On a Form of Loss of Memory occasionally following Cranial Injuries. By JOSEPH BELL, Surgeon to the Edinburgh Royal Infirmary.—Mr. Bell says:—In a certain number of head injuries, in addition to and after recovery from the early symptoms, it is found that the victim has, much to his own surprise, forgotten entirely not the accident itself and the succeeding circumstances only, but a certain length of time varying in different cases from minutes up to hours and even days, with all its actions, pains and pleasures, before the accident happened.

This point has been kept in view in all the cases of head injuries under Mr. Bell's care; and, after excluding all doubtful cases where intoxication or narcotism existed, a certain proportion remain who illustrate the above condition. In order that memory may take place, two things are necessary. 1. The fact or word or thought must be recorded, *i.e.*, some permanent change or impression must be made on brain cells by which it is stored up for future use. 2. When required it must be recalled, *i.e.*, by an act of volition more or less rapid and easy in different people, aided mainly by what we call association, initiated often by other organs of sense, such as sight, smell, or hearing, we bring back, recollect what has been recovered and retained.

The cranial injuries referred to have in some way destroyed the recording power for a time, or at least prevented a certain amount of phenomena being recorded at all. Does this not give us the hint that, for a safe record, a certain amount of time is needed, as a photograph needs a certain amount of exposure, and if this is interfered with, the picture is blurred or wiped out?—*Edinburgh Medical Journal.* February, 1883. —J. C. R.

Macroglossia.—At the Société Médicale des Hôpitaux, on the 9th February, 1883, M. Constantin Paul showed a little girl of three and a-half years of age who, from her birth, has suffered from an enlargement of the tongue, which projects from her mouth. She sucked for one month, and then took the feeding bottle. Now she presents manifest traces of rickets. For the last two years the tongue has developed considerably, and it has now become enormous and œdematous. There are to be seen in it glands, secreting a serous or seropurulent liquid. Mons. Constantin Paul has never met with a similar case in scrofula.—*Gazette des Hôpitaux.* 13th February, 1883, p. 141.—J. L. S.

Osseous Lesions in the Hemiplegic.—M. Debone, having remarked that in hemiplegics fractures almost always affect the paralysed side, examined the bones in some old cases. He found that the bones on the affected side were much lighter than those on the sound side, and a transverse section showed the medullary canal larger, and the compact tissue less thick. Under the microscope the Haversian canals were found increased in size, and the quantity of fat more considerable on the affected side. A true osteoporosis is therefore the cause of the frequency of fractures in the hemiplegic. In the fractures the callus is more abundant, but the consolidation is perhaps even a little more rapid than normal.—*La France Médicale*, 18th October, 1881.—G. S. M.

Books, Pamphlets, &c., Received.

Papers Relating to the Administration of the Dentists' Act.

On the Pathology of Bronchitis, Catarrhal Pneumonia, Tubercle, and Allied Lesions of the Human Body. By D. J. Hamilton, M.B., F.R.C.S.E. With Illustrations. London: Macmillan & Co. 1883.

Transactions of the Obstetrical Society of London. Vol. XXIV. For the Year 1882. With a List of Officers, Fellows, &c. London: Longmans, Green & Co. 1883.

Practical Lessons in Elementary Physiology and Physiological Anatomy. For Schools and Science Classes. By D. M'Alpine, F.C.S. Twelve Plates, with Practical Directions and Explanatory Text. London: Baillière, Tindall & Cox. 1883.

Transfusion: its History, Indications, and Modes of Application. By C. E. Jennings, L.R.C.P.Lond. With Engravings, and a Bibliographical Index. London: Baillière, Tindall & Cox. 1883.

Lectures on Cataract: its Causes, Varieties, and Treatment. By Geo. Cowell, F.R.C.S. With Illustrations. London: Macmillan & Co. 1883.

Mechanical Exercise a Means of Cure, being a Description of the Zander Institute, London. Edited by the Medical Officer to the Institution. London: J. & A. Churchill. 1883.

Die moderne Radikal-operation der Unterleibsbrüche. Eine Statistische Arbeit, von Dr. H. Leisrink, Hamburg. Hamburg & Leipzig: Leopold Voss. 1883.

Die Missenverhältnisse des Menschlichen Herzens; von Wilhelm Müller, Jena. Hamburg & Leipzig: Leopold Voss. 1883.

A System of Surgery, Theoretical and Practical, in Treatises by various authors. Edited by T. Holmes, M.A.Cantab., and J. W. Hulke, F.R.S. Third edition. In Three Volumes. With Illustrations. London: Longmans, Green & Co. 1883.

A Manual of Pathology. By Joseph Coats, M.D. With three hundred and thirty-nine Illustrations. London: Longmans, Green & Co. 1883.

What to do in Cases of Poisoning. By Wm. Murrell, M.D. Third edition. London: H. K. Lewis. 1883.

Materia Medica: a Manual for Students. By Isambard Owen, M.D. London: J. & A. Churchill. 1883.

Tables of Materia Medica: a Companion to the Materia Medica Museum. By T. Lauder Brunton, M.D., Sc.D., F.R.S. New edition. London: Macmillan & Co. 1883.

Elements of Histology. By E. Klein, M.D., F.R.S. Illustrated with 181 Engravings. London: Cassell & Company (Limited). 1883.

Working Bulletins for the Scientific Investigation of the Newer Materia Medica. Vol. I. Issued by the Scientific Department of the Laboratory of Parke, Davis & Co., Manufacturing Chemists, Detroit, Michigan, U.S.A. 1883.

The Untoward Effects of Drugs: a Pharmacological and Clinical Manual. By Dr. L. Lewin, Berlin. Second edition. Revised and enlarged. Translated by J. J. Mulheron, M.D., Detroit, Michigan. George S. Davis. 1883.

A Synoptical Guide to the Study of Obstetrics. By Robert Barnes, M.D. London: Smith, Elder & Co. 1883.

Auscultation and Percussion, together with the other Methods of Physical Examination of the Chest. By Samuel Gee, M.D. Third edition. London: Smith, Elder & Co. 1883.

The Students' Manual of Venereal Diseases. By Berkeley Hill and Arthur Cooper. Third edition. London: Smith, Elder & Co. 1883.

A Guide to Therapeutics. By Robert Farquharson, M.P., M.D. Third edition. London: Smith, Elder & Co. 1883.

Official Report of the Smoke Abatement Committee, 1882, with Reports of the Jurors of the Exhibition at South Kensington, &c., to which are added the Official Reports of the Manchester Exhibition, and 76 Plates of Illustrations, &c., &c. London: Smith, Elder & Co. 1883.

Indigestion, Biliousness, and Gout in its Protean Aspects. Part II. Gout in its Protean Aspects. By J. Milner Fothergill, M.D. London: H. K. Lewis. 1883.

Transactions of the New York Academy of Medicine, instituted 1847. Second series. Vol. III. Printed for the Academy. 1883.

INDEX.

Abscess of head of tibia, 156.
Advances in surgery during the last decade. By Dr. Jas. Whitson, 6.
Addison's disease, case of, 461.
Anæsthesia, mixed, 316.
Anæsthetic mixtures for small operations, 477.
Anasarca, funnel-drainage in, 164.
Anderson, Prof. M'Call.—On the treatment of pulmonary consumption, 81.
Aneurism of aorta, 60.
Anti-vivisection meeting in Manchester, 216.
Aural diagnosis; introductory lecture. By Dr. J. P. Cassells, 281.
Aural diagnosis. By Dr. J. P. Cassells, 432.
Aural pathology, lecture introductory to a course on. By Dr. J. P. Cassells, 20.

Bacillus of tubercle: its relation to phthisis pulmonalis. By J. Lindsay Steven, M.B., 344.
Baginsky, Dr. A.—Lehrbuch der Kinderkrankheiten (*Review*), 143.
Balmer and Fräntzel.—On the tubercle bacillus, &c., 395.
Barbour, Mr. A. H. and Dr. D. B. Hart.—Manual of Gynecology (*Review*), 218.
Bi-chromate of potash poisoning. By E. O. Macniven, M.B., C.M., 373.
Blindness, programme of a prize essay on the prevention of, 139.
Brain lesions determining general or local contractures; experiments, 167.
Bristowe, Dr. J. S.—A treatise on the theory and practice of medicine (*Review*), 388.
Brock, Mr. W. J.—On obstinate vomiting in pregnancy, 198.
Bromide of ethyl during labour, 247.
Brown, Dr. W. Symington.—A clinical handbook on the diseases of women (*Review*), 288.
Buchanan, Prof. G.—On a case of acute fœtid empyema, treated by incision into pleural cavity, with copious ablution; rapid recovery, 134.
Burdett, H. C.—The relative mortality after amputation of large and small hospitals (*Review*), 296.

Carotid, common, ligature of, 389.
Cassells, Dr. J. P.—A lecture introductory to a course of aural pathology, 20.
  A lecture introductory to a course of aural diagnosis, 281.
  Aural diagnosis, 432.
Cerebro-spinal meningitis, case of, 459.
Charbon, virus of symptomatic, 474.
Cheyne, Mr. W. W.—Antiseptic surgery: its principles, practice, history, and results (*Review*), 57.
  Relation of micro-organisms to tuberculosis, 403.
Chloral in diphtheria, 79.
Chlorate of potash as a therapeutic agent. By Dr. J. S. Main, 2.7.
Cinchonism, a victim's account of, 316.
Coats, Dr. J.—On Koch's researches on tuberculosis, 160.
  A manual of pathology (*Review*), 442.
Concussion of the chest, 166.
Contagious Diseases Acts and Government Lock Hospitals, a defence of. Mr. F. W. Lowndes, 33, 169.
Contagious Diseases Acts and Government Lock Hospitals, an exposure of. By Dr A. Patterson, 105. (See also last vol. of *Journal*, p. 401.)
Corrosive sublimate as an antiseptic surgical dressing, 241.
Cramer, Dr. Fr.—On tubercle bacillus, &c., 398.

Death, sudden, during labour, 319.
Dettweiler and **Meissen** on tubercle bacillus, &c., 400.
Diphtheria, chloral in, 79.
Diverticula of urinary bladder, 72.
Doses, frequent repetition of, 321.
Dreschfeld, Dr. J.—On tubercle bacillus, &c., 400.
Dressing, new, for large ulcers. By Dr. A. Pearson, 377.

Empyema, acute, fœtid, treated by incision into pleural cavity, with copious ablution. By Prof. G. Buchanan, 134.
Endocarditis in diabetes, 165.
Enteric fever; case with two relapses, with unusual prolongation of interval between first and second attacks (with chart). By R. W. Forrest. M.D., 368.
Epistaxis treated by warm water, 167.
Erysipelas, pathology of. By Fehleisen, 468.
Esmarch, Dr. F.—First aid to the injured (*Review*), 151.
Euphorbia lathyris, therapeutical value of the seeds of, 79.
Excision of tongue, partial; six cases of, 229.
Exophthalmic goitre, treated by subcutaneous injection of ergotine, 165.

FINLAYSON, DR. J.—Case of simple perforating ulcer of the œsophagus opening into the left bronchus; gangrene of lung, 313.
Floating kidney treated by fixation, 380.
Foramen ovale, patent, 72.
FORMAN, DR. H. F.—On tubercle bacillus, &c., 397.
FORREST, R. W., M.D.—Notes of a case of enteric fever which had two relapses, with an unusual prolongation of the interval between the first and second attacks (with chart of temperatures), 368.

GAIRDNER, PROF. W. T.—On Koch's researches on tuberculosis, 162.
General Medical Council, 215.
Germs and the spray, 320.
Glasgow and West of Scotland Medical Association; editor's report, 138.
Gonococcus, the germ of gonorrhœa, 470.
GRAY, HENRY, F.R.S.—Anatomy, descriptive and surgical (*Review*), 383.
GUTTMANN on tubercle bacillus, &c., 394.

HAHN'S operation for excision of knee, 472.
HART, DR. D. B. and MR. A. H. BARBOUR.—Manual of gynecology (*Review*), 218.
HART, MRS. ERNEST.—Note on the formation of fibrine, &c. (*Review*), 223.
HENDERSON, DR. F.—A case of double uterus in the living subject, 268.
Hernia; pathology of subcutaneous operation by injection. By Joseph H. Warren, A.M., M.D., 260, 329.
Hernia, radical cure, 63.
Hernia, supposed, in lower part of thorax: report of committee, 68.
HERON, DR. G. A.—On Koch's researches on tuberculosis, 94.
HILL, MR. BERKELEY.—The essentials of bandaging (*Review*), 298.
HILLER.—On tubercle bacillus, &c., 394.
HIRSCHFELDER, DR.—On tubercle bacillus, &c., 401.
Hydronephrosis, 73.

INDEX catalogue of library of Surgeon-General's office, United States Army, vol. iii (*Review*), 153.
International Encyclopædia of Surgery, vol. ii (*Review*), 47.
International Medical Congress, 1884, 215.

KNEE, excision of; Hahn's operation, 472.
KNOX, D. N., M.A., M.B.—On two cases of laryngeal tumour, in one of which the tumour was successfully removed by thyrotomy, 249.
KOCH, DR. R.—On tubercle bacillus, &c., 401.
Koch's researches on tuberculosis. By DR. G. A. Heron, 94.

LAPARO-COLOTOMY for removal of a foreign body, 248.
Laryngeal cancer, primitive, 319.
Laryngeal tumour, notes of two cases of, in one of which the tumour was successfully removed by thyrotomy. By D. N. Knox, M.A., M.B., 249.
LICHTHEIM.—On tubercle bacillus, &c., 395.
LOWNDES, MR. F. W.—A defence of the Contagious Diseases Acts and of Government Lock Hospitals, 33, 169.

MACNIVEN, E. O., M.B., C.M.—On bi-chromate of potash poisoning, 373.
Macroglossia, 478.
MAIN, DR. J. S.—Chlorate of potash as a therapeutic agent, 277.
Note on increased resistance on percussion as an aid to diagnosis, 32.
Malformations, complicated; a case. By Dr J. K. Robertson, 96.
MALLEY, MR. A. COWLEY, B.A.—Micro-photography (*Review*), 455.
Malignant periostitis and pyæmia without external wound, 305.
MARSHALL, DR. ANDREW.—A case of impermeable stricture of the urethra treated by perinæal section according to Wheelhouse's plan, 18.
MARSHALL, DR. A. M.—Owen's College course of elementary biology (*Review*), 392.
Mastitis, puerperal, treated by iodide of lead ointment, 475.
Medical student, the, 80.
Medico-Chirurgical Society, meetings of, 159, 306, 308.
MEDICO-CHIRURGICAL SOCIETY, PROCEEDINGS OF—
  HERON, DR. G. A.—Koch's observations on the tubercle bacillus, 159.
  WHITSON, DR. J.—(1) Case of adenoid sarcoma of mamma; (2) Case of compound fracture of clavicle; (3) Case of radical cure of hernia, 306.
  SMITH, DR. A. WOOD.—Notes of a case of catalepsy, 308.
  FLEMING, DR. W. J.—Substitute for the jury-mast in the treatment of spinal disease, 310.
Memory, loss of, after cranial injuries, 478.
MENCHE, DR. H.—On tubercle bacillus, &c., 398.
Mercurial and non-mercurial treatment of syphilis. By Dr. R. Park, 189.
Milk indigestion in young infants, 167.
MUNRO, R., M.A., M.D.—On minute organisms and their relation to disease, 355, 409.

NAPIER, DR. A.—Case of purpura, 158.
Vaccinal eruptions, 424.
Nausea and vomiting in uterine affections, treatment of, 245.
Nephroraphy performed for first time in this country, 380.
Nerve-stretching, subsequent history of cases of, 168.
Nitrite of amyl in opium poisoning, 75.
NORRIS, DR. R.—Physiology and pathology of the blood (*Review*), 223.

OBITUARY—
  J. G. LYON, M.D., 141.
  PETER STEWART, M.D., 440.
Œsophageal stricture, malignant, with gangrene of the lung, 391.
OGLE, DR. J. W.—The Harveian oration, 1880 (*Review*), 53.
Organisms, minute, and their relation to disease. By Dr. R. Munro, 355, 409.
Osseous lesions in the hemiplegic, 479.
Otorrhœa, dry local treatment in, 244.

Owen, Dr. J.—Supplementary catalogue of the pathological museum of St. George's Hospital (*Review*), 388.
Oxygenated water, use of, 477.

Park, Dr. R.—On the mercurial and non-mercurial treatment of syphilis, 189.
Patent foramen ovale, 72.
Pathological anatomy of tuberculosis of the uterus and Fallopian tubes. By J. Lindsay Steven, M.B., 1.
Pathological and Clinical Society, meetings of, 68, 310.
Pathological and Clinical Society, Proceedings of—
- Committee report on a case of supposed hernia in lower part of thorax, 68.
- Newman, Dr. D.—Patent foramen ovale; perforation of septum ventriculorum, 72.
- Coats, Dr. J.—Diverticula of urinary bladder, 72.
  - Hydronephrosis, 73.
- Clark, Mr. H. E.—Congenital sarcoma of abdomen, 235.
  - Scirrhus of testicle and spermatic cord, 236.
- Coats, Dr. J., and Knox, Dr. D. N.—Papilloma of larynx, 237.
- Knox, Dr. D. N.—Epithelioma of larynx, 238.
- Renton, Dr. J. C.—Myoma of uterus, 238.
  - Diseased knee-joint and portion of femur, 310.
- Beatson, Dr. G. T.—Case of excision of patella, 311.
- Cameron, Dr. H. C.—Transverse fracture of the patella, 311.
- Pollok, Dr. R.—Ovarian cyst, 312.
- Finlayson, Dr. J.—Ulcer of œsophagus opening into left bronchus: gangrene of lungs, 313.

Patterson, Dr. A.—An exposure of the Contagious Diseases Acts and of Government Lock Hospitals, 105.
Pearson, Dr. Archibald.—A new dressing for large ulcers, 377.
Pelvic peritonitis, 155.
Periostitic abscess of lower jaw; spontaneous hæmorrhage; ligature of common carotid: death, 389.
Pfeiffer, Dr. A.—On tubercle bacillus, 397.
Phenoresorcine, 315.
Photography of microscopic sections. By Dr. J. Whitson, 211.
Phthisis, contagiousness of, 475.
Puerperal mastitis treated by iodide of lead ointment, 475.
Pulmonary consumption, treatment of. An address delivered at the Pathological and Clinical Society. By Professor M'Call Anderson, 81.
Pulmonary hæmorrhage, simple means of checking, 474.
Purpura, 158.

Quain, Dr. R.—A dictionary of medicine (*Review*), 294.

Ransome, Dr. A.—On tubercle bacillus, &c., 398.
Rectal examination in hip-joint disease, 78.
Rectum, prolapse of, treated by hypodermic injections of ergotine, 315.
Reeves, Mr. H. A.—Human morphology (*Review*), 147.
Resection of sternum, 245.
Resistance on percussion as an aid to diagnosis. By Dr. J. S. Main, 32.
Reviews—
- Baginsky, Dr. A.—Lehrbuch der Kinderkrankheiten, 143.
- Bristowe, Dr. J. S.—Theory and practice of medicine, 388.
- Brown, Dr. W. Symington.—Clinical handbook on the diseases of women, 288.
- Burdett, H. C.—Relative mortality after amputations of large and small hospitals, 296.
- Cheyne, Mr. W. W.—Antiseptic surgery, 67.
- Coats, Dr. Joseph.—A manual of pathology, 442.
- Esmarch, Dr. F.—First aid to the injured, 151.
- Gray, Henry, F.R.S.—Anatomy, descriptive and surgical, 383.
- Hart, Mrs. E.—Formation of fibrine, &c., [illegible].
- Hart, Dr. D. B., and Barbour, Mr. A. H.—Manual of Gynecology, 218.
- Hill, Mr. Berkeley.—Essentials of bandaging, 298.
- Index Catalogue of Library of Surgeon-General's Office, United States Army, vol. iii, 153.
- International Encyclopædia of Surgery, vol. ii, 47.
- Malley, Mr. A. C.—Micro-photography, 455.
- Marshall, Dr. A. M.—Elementary biology: the frog, 302.
- Norris, Dr. R.—Physiology and pathology of the blood, 223.
- Ogle, Dr. J. W.—Harveian oration, 1880, 53.
- Owen, Dr. J.—Supplementary catalogue of pathological museum of St. George's Hospital, 388.
- Quain, Dr. R.—Dictionary of Medicine, 294.
- Reeves, Mr. H. A.—Human morphology, 147.
- St. Thomas' Hospital reports, 389.
- Smithsonian Institution.—Annual reports for 1878, 1879, and 1880, 299.
- Southam, Mr. F. A.—Regional Surgery, 301.
- Thompson, Sir Henry.—Diseases of the prostate, 458.
- Tidy, Ch. M.—Legal medicine, part i, 291.
- Wooton, E.—Guide to the medical profession, 386.

Rheumatic œdema and supra-clavicular pseudo-lipoma, 317.
Rindfleisch, Prof.—Method of staining tubercle bacillus, 402.
Robertson, Dr. J. K.—On a case of complicated malformations, 26.
Rosenstein; tubercle bacillus in urine, 402.

Salicylate of soda; action on heart, 476.
Scirrhus of testicle and spermatic cord, 67.
Sciatica, nature of, 78.
Septum ventriculorum, perforation of, 72.
Sequestra, absorption of, 318.
Skull, compound comminuted fracture of, with hemiplegia, 303.
Smith, Dr. R. Charnley.—On tubercle bacillus, &c., 401.

Smithsonian Institution. Annual reports, 1878, 1879, 1880 (*Review*), 299.
Southam, Mr. F. A.—Regional Surgery (*Review*), 301.
Southern Medical Society, meetings of, 239, 466.
Southern Medical Society, proceedings of—
  Park, Dr. R.—Treatment of syphilis, 239.
  Napier, Dr. A.—Vaccinal eruptions, 466.
Spina, Dr. A.—On tubercle bacillus, &c., 400.
Steven, J. Lindsay, M.B.—On the pathological anatomy of tuberculosis of the uterus and Fallopian tubes, 1.
  On the tubercular bacillus and its relation to phthisis pulmonalis, 344.
St. Thomas's Hospital reports (*Review*), 389.
Stricture of the urethra, impermeable, treated by perineal section, according to Wheelhouse's plan. By Dr. A. Marshall, 18.

Thompson, Sir Henry. Diseases of the prostate (*Review*), 458.
Thyrotomy; operation by D. N. Knox, M.A., M.B., 249.
Tidy, Mr. Ch. Meymott.—Legal medicine, part i (*Review*), 291.
Tissue repair, or the pathology of the subcutaneous operation by injection for the cure of hernia. By Joseph H. Warren, A.M., M.D., 260, 329.
Tracheotomy, sequelæ of, after closure of the tracheal wound, 320.
Transplantation of muscle, 75.
Trendelenburg's method of amputation at the hip joint, 380.
Tubercle and inflammation, relations of, 324.
Tubercle bacillus, abstract of observations on, by Hiller, Guttmann, Balmer and Fräntzel, Lichtheim, Pfeiffer, Formad, Ransome, Ziehl, Menche, Crämer, Dreschfeld, Dettweiller and Meissen, Spina, Koch, Hirschfelder, Charnley Smith, Rindfleisch, Rosenstein, Watson Cheyne, 394.
Tubercular bacillus and its relation to phthisis pulmonalis. By J. Lindsay Steven, M.B., 344.
Tuberculosis, Koch's researches on. By Dr G. A. Heron, 94.
Tuberculosis of uterus and Fallopian tubes, pathological anatomy of. By J. Lindsay Steven, M.B., 1

Ulcers, large; a new dressing for. By Dr A. Pearson, 377.
Unconsciousness, sudden; diagnosis of cause, 75.
Urethral syringe, Mr Balmanno Squire's, 2
Urinary bladder, diverticula of, 72
Uterine cancer, treatment of, 32
Uterine displacements, new operation for the cure of, 246.
Uterus, double, in the living subject. By Dr F. Henderson, 268.

Vaccinal eruptions. By Dr. A. Napier, 424.
Vaccinal eruptions (*Discussion*), 466.
Venesection. By John Wylie, M.B., 256.
Voluntary Lock Hospitals and the Contagious Diseases Acts. By F. W. Lowndes, M.R.C.S.E., 33.
Vomiting, obstinate, in pregnancy. By Mr W. J. Brock, 198.

Warren, Dr. J. H. Tissue repair, or the pathology of the subcutaneous operation by injection for the cure of hernia, 260, 329.
Whitson, Dr. Jas.—On some of the advances made in surgery during the last decade,
  On the photography of microscopic sections, 211.
Wooton, Edwin.—A guide to the medical profession (*Review*), 386.
Wylie, John, M.B.—On venesection, 256.

Ziehl, Dr. F.—On tubercle bacillus, &c., 39

GLASGOW: PRINTED BY ALEX. MACDOUGALL.

SERIAL

R
31
G5
v.19

Glasgow medical journal

GERSTS